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C O N T E N T S.

No. LXXIII.—AUGUST 1, 1866.

PART I.—ORIGINAL COMMUNICATIONS.

	PAGE
ART. 1.—On the Operation of Trephining in Cases of Fracture of the Spine. By ROBERT M'DONNELL, M.D., F.R.S.; Surgeon to Jervis-street Hospital; Lecturer on Surgery in the Carmichael School of Medicine; Examiner in Surgery in the Queen's University. (Illustrated),	1
ART. 2.—On the Action of the Heart. By GEORGE PATON, M.D., Toronto,	35
ART. 3.—Temperature in Acute Disease. Being a Thesis read, for the Degree of Doctor in Medicine, before the University of Dublin, June 26th, 1866. By THOMAS ARMETIRDING COMPTON, B.A. of Corpus Christi College, Cambridge; B.A. <i>ad eundem</i> , Dubl., and M.B., Dubl.; L.R.C.P., London; M.R.C.S., Eng.,	60
ART. 4.—The Medical Uses of Chloroform Inhalation. By CHARLES KIDD, M.D.,; M.R.C.S.E., London,	83
ART. 5.—Diphtheria; a Paper Read before the Medical Society of the College of Physicians, Ireland, March 21, 1866. By DR. HAYDEN, Physician to the Mater Misericordiæ Hospital,	86
ART. 6.—Reports of Hospital Cases:—Strangulated Inguinal Hernia. By WILLIAM MAC CORMAC, M.A., M.D., Q.U.I., F.R.C.S.I.; Surgeon, Belfast General Hospital,	94

PART II.—REVIEWS AND BIBLIOGRAPHICAL NOTICES.

1. Clinical Notes on Uterine Surgery, with Special Reference to the Management of the Sterile Condition. By J. MARION SIMS, A.B.; M.D., 107
2. Collection of Memoirs concerning a Function of the Pancreas Little Known, viz., the Digestion of Azotized Aliment. By LUCIEN CORVISART, 120
3. On the Nature, Cause, and Treatment of Tuberculosis. By HORACE DOBELL, M.D., 120
4. On the Use of the Sphygmograph in the Investigation of Disease. By B. W. FOSTER, M.D. (Illustrated), 125
5. New Observations on the Structure and Formation of Certain Nervous Centres, tending to Prove that the Cells and Fibres of every Nervous Apparatus form an Uninterrupted Circuit. By LIONEL S. BEALE, M.B., F.R.S., 128
6. Recent Works on Cholera:—
 - i. The Common Nature of Epidemics. By SOUTHWOOD SMITH, M.D., 132
 - ii. Notes on Cholera, its Nature and Treatment. By GEORGE JOHNSON, M.D., 132
 - iii. Cholera in its Home. By JOHN MACPHERSON, M.D., 132
 - iv. Cholera—its Symptoms, Causes, and Remedies. By THOMAS A. WISE, M.D., 132
 - v. Le Choléra ou Typhus Indien Epidémie de 1865. Par le Docteur CHARLES PELLARIN, 132
 - vi. Le Choléra et Le Congrès Sanitaire Diplomatique International. Par Le Docteur J. P. BONNAFONT, 132
 - vii. Cholera. By WILLIAM STORY, 132
 - viii. Essay on the Nature and Treatment of Cholera and Fever, with Medical Remarks on the Treatment of Cattle Plague. By JAMES TUCKER, M.D., 132
7. Observations on the Functions of the Liver, more especially with Reference to the Formation of the Material known as Amyloid Substance, or Animal Dextrine, and the Ultimate Destination of this Substance in the Animal Economy. By ROBERT M'DONNELL, M.D., M.R.I.A., 162
8. Atlas of Surgical and Topographical Anatomy. By B. J. BÉRAUD, 167

PART III.—MEDICAL MISCELLANY.

Proceedings of the Dublin Pathological Society:—

- | | |
|--|-----|
| DR. HAYDEN on Visceral Cancer, | 168 |
| DR. M'DONNELL on Chronic Laryngitis, | 171 |
| MR. CROLY on Necrosis of the Lower Jaw, | 172 |
| DR. M'DONNELL on Fracture of the Skull, | 173 |
| DR. BENNETT on Cardiac Disease—Patent Foramen Ovale, | 174 |
| DR. HAYDEN on Cardiac Disease, Mitral Obstruction, Aortic Regurgitation, | 177 |

Contents.

iii

PAGE

Proceedings of the Dublin Obstetrical Society:—

DR. DENHAM on a Case of Inversion of the Uterus after Delivery,	181
DR. WILSON on Ophthalmia of New-born Children,	184
DR. M'CLINTOCK on a Case of Double Monstrosity,	192

Transactions of the County and City of Cork Medical and Surgical Society:—

DR. MACNAUGHTON on Digitalis, and its Uses in Medical Practice,	194
DR. TOWNSEND, jun., on a Remarkable Case of Pneumothorax, without Perforation,	202

Transactions of the Medical Society of the College of Physicians:—

DR. BYRNE on a Case of Puerperal Fetid Pulmonary Abscess,	210
DR. M'SWINEY on a Case of Traumatic (?) Tetanus,	219
DR. MADDEN on Insanity and Criminal Responsibility, &c.	224

Memoir of Sir Patrick Dun. By T. W. BELCHER, M.D.,	231
--	-----

Notices to Correspondents,	iii
--------------------------------------	-----

Books Received,	295
---------------------------	-----

Exchange Journals,	15
------------------------------	----

NOTICES TO CORRESPONDENTS.

We have been obliged to hold over several Original Communications, Reviews, and Clinical Records.

Authors of Communications are requested to write the prescriptions in their papers, in full, and in English.

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N.B.—With the present number we give 48 additional pages, so as to publish in a complete form DR. BELCHER's learned "Memoir of Sir Patrick Dun."

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C O N T E N T S.

No. LXXIV.—NOVEMBER 1, 1866.

PART I.—ORIGINAL COMMUNICATIONS.

	PAGE
ART. VII.—On Amputation at the Hip-joint. By RICHARD G. BUTCHER, Esq., President of the Royal College of Surgeons in Ireland; Honorary M.D. of the University of Dublin; Associate-Fellow of the College of Physicians of Philadelphia; Surgeon to Mercer's Hospital, Dublin; and Lecturer on Clinical Surgery (Illustrated), .	297
ART. VIII.—Contributions to Operative Surgery. Part I.—Operations about the Face. By MAURICE HENRY COLLIS, M.B., Univ., Dub.; F.R.C.S.I.; Surgeon to the Meath Hospital and County Dublin Infirmary; Sometime Examiner in Surgery to the Queen's University in Ireland, and Member of Council of the Royal College of Surgeons in Ireland, &c. (Illustrated),	324
ART. IX.—Ophthalmoscopic Notes.—On the Theory and Principles of the Ophthalmoscope. By HENRY WILSON, F.R.C.S.I.; L.K. & Q.C.P.I.; Assistant-Surgeon to St. Mark's Ophthalmic Hospital; Member of the Royal Irish Academy, &c. (Illustrated),	345
ART. X.—On the Use of Sulphite of Magnesia in the Treatment of Zymotic Diseases. By H. R. DE RICCI,	360
ART. XI.—Report on Scalds of the Larynx. By PHILIP BEVAN, M.D., T.C.D.; F.R.C.S.I.; Surgeon to Mercer's Hospital,	367
ART. XII.—Case of Protracted Utero-Gestation; with Remarks. By C. JOYNT, M.D.; L.K. & Q.C.P., &c.; Assistant-Surgeon 14th Bo. N.I.; late Professor of Midwifery, Grant Medical College; Examiner in Medicine and in Medical Jurisprudence, University of Bombay; and Surgeon to the Bombay Gaol, and to the House of Correction,	377
ART. XIII.—Flax Mills—their Machinery; Accidents Occurring Therein with Suggestions for their Prevention. By THOMAS H. BABINGTON, M.D.; T.C.D.; M.R.L.A.; Surgeon to County Londonderry Infirmary,	392

	PAGE
PART II.—REVIEWS AND BIBLIOGRAPHICAL NOTICES.	
1. Contributions to Medicine and Midwifery. By THOMAS EDWARD BEATTY, M.D.,	397
2. Recent Works on Cholera :—	
I. The Antidotal Treatment of the Epidemic Cholera. By JOHN PARKIN, M.D.,	403
II. The Nature of Cholera, as a Guide to Treatment. By WILLIAM SEDGWICK, M.R.C.S., &c.,	403
III. On Cholera, its Nature and Treatment; being the Debate in the Harveian Medical Society of London. Edited by DR. C. DRYSDALE,	403
IV. Cholera; A New Theory. By C. DUDLEY KINGSFORD, M.D.,	403
V. Brief Remarks on Cholera. By ROBERT J. SPITTA, M.D.,	403
VI. On the Treatment of Asiatic Cholera. By ARCHIBALD BILLING, M.D.,	403
VII. Mechanical Treatment of Cholera. By A PHYSICIAN,	403
VIII. The Human Blight and Cattle Blight,	403
IX. On Epidemic Diarrhea and Cholera, their Nature and Treatment. By GEORGE JOHNSON, M.D.,	403
X. The Arrest and Prevention of Cholera. By ARTHUR ERNEST SANSOM, M.B.,	403
3. Note-Book of Materia Medica, Pharmacology, and Therapeutics. By R. E. SCORESBY-JACKSON, M.D.,	411
4. Memoir of Sir Patrick Dun. By T. W. BELCHER, M.D.,	413
5. Recent Works on the Microscope :—	
I. A Treatise on the Construction, Proper Use, and Capabilities of Smith, Beck, and Beck's Achromatic Microscope. By RICHARD BECK,	414
II. Smith, Beck, and Beck's Illustrated Catalogue,	414
III. An Elementary Text-Book of the Microscope; including a Description of the Methods of Preparing and Mounting Objects, &c. By J. W. GRIFFITH, M.D.,	414
6. Army Hygiene. By CHARLES ALEXANDER GORDON, M.D.,	416
7. Fissure of the Soft and Hard Palate. By J. MASON WARREN, M.D.,	424
8. The Malformations, Diseases, and Injuries of the Fingers and Toes, and their Surgical Treatment. By THOMAS ANNANDALE, F.R.C.S.,	426
9. Symptoms and Diagnosis of Tumours of the Brain. By DR. PAUL LADAME,	435
10. Codex Medicamentarius: French Pharmacopeia,	447
11. Tetanus.—A Physiological Study. By DR. JOHANNES RANKE,	450
12. Surgical Experiences:—The Substance of Clinical Lectures. By SAMUEL SOLLY, F.R.S.,	463
13. Works on Cancer :—	
I. The Antecedents of Cancer. By CHAS. H. MOORE, F.R.C.S.,	466
II. Primary Cancer of the Brain. By G. MACKENZIE BACON, M.D.,	466
III. On Intra-Thoracic Cancer. By JOHN COCKLE, M.D.,	466
IV. On Cancer, its Allies and Counterfeits. By THOS. WEEDEN COOKE, Surgeon,	466

PART III.—MEDICAL MISCELLANY.

	PAGE
Proceedings of the Dublin Pathological Society:—	
MR. E. HAMILTON on Intra-thoracic Aneurism, . . .	474
DR. PURSER on Amyloid Degeneration, . . .	475
DR. MARCUS EUSTACE on Ossification of the Dura Mater, . . .	478
DR. FLEMING on Vesical Calculus, . . .	480
DR. LYONS on Bright's Disease of the Kidney, . . .	482
MR. SYMES on Separation of the Symphysis Pubis, with Rupture of the Bladder, . . .	485
DR. FLEMING on Polypus of the Rectum, . . .	487
DR. BANKS on Hemiplegia, with Loss of Speech, . . .	488
DR. DUNCAN on Hepatic Abscess, . . .	491
DR. BENNETT on Tumour of the Uterus, . . .	493
DR. PURSER on Staphyloma, . . .	495
DR. SYMES on Laceration of the Duodenum, . . .	497
DR. DENHAM on Tumour of the Uterus, . . .	498
DR. FLEMING on Fracture of the Pelvis—Rupture of the Bladder, . . .	499
DR. BARTON on Obstructions of the Urethra, . . .	501
DR. PURSER on Amyloid Degeneration of the Liver, . . .	502
DR. JENNINGS on Diseases of the Mitral, Aortic, and Tricuspid Valves, . . .	504
DR. JENNINGS on Disease of the Mitral Valves, . . .	506
DR. HAYDEN on Softening of the Brain, . . .	507
DR. LAW on Obesity of the Heart, . . .	511
DR. HAYDEN on Aneurism of the Aorta, . . .	512
DR. JENNINGS on Disease of the Mitral and Aortic Valves; Hypertrophy of the Left Ventricle, . . .	517
DR. M'SWINEY on Rupture of the Biliary Duct, . . .	518
DR. HAYDEN on Cirrhosis of the Liver, . . .	519
Transactions of the Medical Society of the College of Physicians:—	
DR. MADDEN on Insanity and Criminal Responsibility, &c., . . .	523
Proceedings of the Dublin Obstetrical Society:—	
DR. BYRNE on Rupture of the Vagina during Labour, . . .	537
DR. ISDELL on Rupture of the Uterus, . . .	537
DR. ISDELL on Placenta Previa, . . .	537
DR. RINGLAND on Pregnancy Complicated with Subacute Peritonitis and Ascites, . . .	537
DR. M'CLINTOCK on Double Monstrosity, . . .	537
DR. KIDD on Medicated Pessaries, . . .	537
DR. JOHN A. BYRNE on Large Fibrous Intestinal Tumour of Uterus, . . .	539

	PAGE
Transactions of the County and City of Cork Medical and Surgical Society :—	
DR. MACNAUGHTON JONES on Abnormal Kidney,	541
DR. JOHNSTON on Pathological Specimens,	542
Clinical Records :—	
DR. T. H. BABINGTON—Case of Poisonous Effects Produced by Exposure to the Fumes of Tobacco, and Excessive Use of “the Weed,”	544
Exchange Journals,	i
Books Received,	iii
Notices to Correspondents,	xii

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AUGUST 1, 1866.

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ORIGINAL COMMUNICATIONS.

ART. I.—*On the Operation of Trephining in Cases of Fracture of the Spine.* By ROBERT M'DONNELL, M.D., F.R.S.; Surgeon to Jervis-street Hospital; Lecturer on Surgery in the Carmichael School of Medicine; Examiner in Surgery in the Queen's University.

IN the number of this Journal, published at this time last year, I offered some observations on a case of fracture of the spine in which I performed the operation of trephining. I briefly alluded in that communication to a case in which Dr. Gordon had performed this operation. Dr. Gordon's case, which has proved successful as regards the patient's life, was communicated to the Royal Medico-chirurgical Society of London in November last, and as the full details of it will be published in the forthcoming volume of the *Medico-chirurgical Transactions* I shall here only notice the leading features of it.

In my former communication I confined myself to a discussion of the general question as to whether the operation of trephining the spine should be adopted in certain cases, or whether it was to be set aside altogether as an operative procedure, as it practically has been for many years past. I am very well aware that not a

few distinguished surgeons are of opinion that it should be set aside altogether; but I also know that there are many who coincide with me in thinking that this operation should be acknowledged in certain cases to be justifiable; that it should not be taught in our schools as a matter of course—that in all cases of fractured spine it is worse than useless, and not to be thought of; but, that the proceeding of trephining the spine should be permitted to take its place among the legitimate operations of surgery as one which, although formidable, and in most cases not likely to prove successful, nevertheless offers, in certain cases, by far a better prospect of saving life than the system of doing nothing.

In the present paper I purpose to consider the special circumstances and symptoms which appear to indicate cases favourable for the operation, or the reverse; and finally the method of performing the operation in the different regions of the spine. Before, however, entering on the consideration of these topics I may be allowed to add a few observations in reply to some objections which have been raised to the operation of trephining the spine.

CASE I. (Dr. Gordon's Case).—Michael Clinton, a slight and well-made, although rather pallid and wasted, man, of thirty-one years of age, was admitted to the Whitworth Hospital, under Dr. Gordon's care, on May 30th, 1865. For some years he had been employed as an engineer on railways in the United States, and describes himself as having been an active, healthy person up to the 27th of March last, when he met with an accident of which he gives the following account:—While hunting, his horse shied at a fence, and he was flung off into a gripe on his back, which came in contact with some stones; immediately his lower limbs were paralysed; he made an effort to get up, but found that he was completely powerless from the waist down. He was carried home in great pain; for four or five days he had retention of urine, requiring the introduction of the catheter; at this period the distention of the bladder caused him acute pain; after this the urine began to dribble away; it became, as he says, "very offensive, and full of corruption-like matter."

From the first he had paralysis of the sphincter ani, with involuntary passage of feces. For five weeks after the accident he was not conscious of sensation in the lower limbs; he had occasional priapism.

After admission to hospital he was carefully examined; his

condition at that time was noted as follows:—In the lower limbs muscular power is entirely lost so far up as the knees; except the sartorius, the muscles of the thigh have little or no power; he has no power over either the bladder or rectum.

Sensation is lost in both feet; from the ankle to the knee he feels obscurely, the left leg being almost devoid of sensibility. There is hyperesthesia of each thigh—on the right side to so great an extent that he is distressed by rubbing the hand across the surface of it. Below the knee he cannot appreciate the difference between heat and cold: a vessel of warm water, which, applied to the left thigh, is of an agreeable temperature—when applied to the right causes him to cry out that he is burned. He can tell, without seeing them, the position in which the lower limbs are placed. No reflex movements can be excited in any of the muscles below the knees; in each thigh reflex motion can be readily aroused; on the right side the slightest touch of a hair, or even blowing on the surface causes vigorous contractions, especially of the sartorius muscle.

The lower limbs are considerably wasted; there is no œdema of the feet or ankles, which are bedewed with perspiration; the penis and scrotum are large and flabby, but not ulcerated; the orifice of the urethra is inflamed, it is not possible to introduce a double channelled silver catheter (size No. 10); a bed-sore, about the size of half-a-crown, has formed on the back; the urine is alkaline, very muddy and fetid, containing a copious muco-purulent deposit, and tinged with blood.

Upon examining the spinal column, the spinous process of one of the lower vertebræ was found to project considerably. This projection existed at a distance of four inches above a circle made round the body at the level of the umbilicus, and was not movable.

The nature of his case, and of the proposed operative procedure having been explained to the patient, the operation of trephining the spine was undertaken at his request, and was performed on June 3rd, 1865, the patient having been placed under the influence of chloroform. The operation lasted fifty minutes; the amount of blood lost was not considerable.

Fourth day after operation.—Urine became acid.

Fifth day after operation.—Urine again alkaline, scalds him in passing.

Eleventh day.—Had a motion from the bowels, of the passage of which he was perfectly conscious, but unable to control. Urine since last date alkaline; drawn off with catheter.

Fourteenth day.—Urine, when drawn off, acid, and quite free from blood.

Sixteenth day.—Urine slightly alkaline, and continued neutral for three days, when it became acid.

Twenty-sixth day.—Can eject the urine from the bladder; and since the beginning of July the power over the bladder is such as to force the water strongly against the side of the vessel in a full stream, and on one occasion to a distance of between two or three feet; urine persistently acid; the catheter is no longer necessary.

Eight weeks after the operation the patient was able to go out into the open air, reclining on his back in a basket carriage. At this date the bed-sore on the back was quite well, and the wound healed; motion was in an appreciable degree restored in the lower limbs, and sensation much improved; the power over the bladder restored, and the condition of the urine almost natural; the patient's appearance much improved, and his appetite good; a minute portion of bone, weighing a couple of grains, had come away; no abscess had formed; he felt his back so strong that he desired to be allowed to sit up; he could turn himself from side to side readily, and with confidence.

Six months after the operation the report was as follows:—The patient can sit upright without the least discomfort, or feeling of weakness in the back; he makes water, he says, as well as ever he did; he cannot stand, or, of course, make an attempt at walking; and although by forcing he can evacuate the bowels, yet the sphincter ani has not regained its power.

Now (July 9th, 1866) the patient being unable to stand or walk, moves himself about in an invalid chair. He has sought employment as a worker at a steam lathe, at iron-works in the neighbourhood of Dublin.

I must congratulate my friend and colleague, Dr. Gordon, on the measure of success which has attended his operation in this instance. The case is one which will become classical—it has the merit of being, if not the first case of trephining of the spine which has been attended with success as regards the saving of life, recorded in the British Islands, at least the first fully and accurately reported. It has the value which is always attached to a case occurring in an hospital in a large city, and beneath the critical eyes of a large number of students and practitioners.

I conceive that no impartial person will deny that in this case the operative procedure and the improvement which followed it

were related to each other as cause and effect. The diminution of hyperesthesia, and of the exalted reflex action—the improvement in the urine, bed-sore, and bladder—the increase of sensibility below the knees, as well as of motor power in the thighs, followed the operation in such a way as to leave little doubt that they were due to it. It is possible, and even probable, that the subsidence of the hyperesthesia and exalted reflex action of the right thigh may have been due, not to the removal of the portion of bone causing pressure or irritation of the cord, but to the counter-irritation produced by the wound beginning to suppurate. This subsidence of hyperesthesia was the earliest sign of amendment, and in consequence of it the patient got great relief from a distressing symptom; but, possibly, active counter-irritation by blisters or issues might have produced a similar result. But this cannot be said for the disappearance of all muco-purulent deposit, and blood from the urine, and the healing of the bed-sore, with, by degrees, the returning acidity of the urine; the perfect power, also, which the patient now has over the bladder must, I conceive, in fairness be attributed to the operation. It is interesting to observe that this power became restored by steps just reversing the order in which it is lost in such cases. The water, instead of constantly dribbling away, became retained—the catheter became necessary for a time, and it gradually became possible to dispense with it altogether.

This case will, no doubt, be regarded very differently by various individuals; some will say, that after all it proves nothing more than that the operation is one not necessarily fatal, but that the patient would probably have made an equally good recovery if not operated upon. If, however, it be admitted that there is nothing in the operation necessarily fatal—if the case shows that extensive supuration and exfoliation, making convalescence very tedious, if not impossible, are not invariable attendants on the operation, a great step has been gained, and many of the most plausible arguments urged against the operation are answered by this alone. For my own part, however, I am convinced that the patient's life was saved by the operation; he thinks so himself also. He was getting gradually worse up to the time of the operation; after it he steadily improved up to a certain point. Having seen him before the operation, and watched him closely since, I can at least say that I have never met with any case presenting such symptoms as Clinton's, which did not certainly run a fatal course. On the other hand some persons will probably attach to this case even a greater importance

than it deserves—that is to say, greater in comparison with cases which have had a less successful issue. They will in short pay the usual tribute to success, and estimate more highly the single fact, that the patient is living twelve months after the operation, than the several facts revealed in a case where marked amendment in different symptoms took place; and yet a *post mortem* examination ultimately showed the exact nature of the injury, and the connexion between the amendment and the operation itself.

Although I am very sensible of the merits of Dr. Gordon's case, yet, I believe, that the instance in which I myself operated is, on this account, more instructive upon some points than his. I may be pardoned for briefly alluding to it:—

CASE II.^a—Joseph Collins, thirty-eight years of age, was admitted to Jervis-street Hospital on December 28th, 1864, having had his spine fractured by a sack of wheat falling upon him from a height; he had the usual symptoms which accompany this injury.

The posterior arch of the first lumbar vertebra was removed by operation, on February 3rd, 1865.

On the day following the operation a great improvement had taken place in the returning sensibility, and power of motion in the lower limbs. The bed-sores which had formed gradually made good progress; the penis and scrotum, which were œdematous and ulcerated, became reduced in size, and the ulcerations healed; the patient regained some power over the bladder.

He died on the 17th day after the operation, in consequence of abscesses having formed in the kidneys.

The *post mortem* examination also showed that the body of the first lumbar vertebra was fractured, and this vertebra was displaced backwards; the line of fracture separated only a small portion of the body of the broken vertebra. The intervertebral substance between the last dorsal and first lumbar vertebræ had been torn. The dura mater of the cord was uninjured; the portion of it corresponding to the piece of bone removed at the time of the operation was covered externally with lymph; the surface next the cord was healthy; there was no trace of inflammation within the dura mater. The cord was not inflamed or softened; it was indented at a point corresponding to where the bone was displaced.

Although in this case the operation would have been much more

^a For full details of the case, see the number of this Journal, August, 1865.

satisfactory if the posterior arch of the last dorsal vertebra had also been removed, yet what was done is sufficient to prove that where pressure on the cord is caused by fracture and displacement of the body of the vertebra, benefit arises from taking away the counter-pressure posteriorly; that, in short, removal of the counter-pressure in such cases is enough to enable the spinal cord, in a great degree, to resume its functions. Of course the fact that benefit is gained by taking away the posterior arches, when the cord is subject to compression, in consequence of fracture or displacement of the vertebral body—that motion and sensation in such cases, more or less, completely returns, and ulcerations heal—in short, the evidence thus offered, that the spinal cord, to a certain extent, recovers its powers is more conclusive than any theoretic argument that can be offered on the subject. The following experiment, however, suggested to me by my former pupil, Dr. Alexander Filson, makes very clear the precise mode in which the removal of the posterior arch may relieve pressure on the spinal marrow, even when arising from some cause acting on the anterior portion of the canal. Suppose that a short piece of indian rubber tube, just large enough

Fig. 1.

to fill it, is introduced into the spinal canal, and the body of the vertebra then crushed or broken with a hammer, in such a manner as is shown in the accompanying woodcut—the Indian rubber tube is compressed. If, then, the laminae be sawn through, and the posterior arch taken away, the indian rubber tube at once recovers its former shape and dimensions. The spinal marrow is not to be supposed to recover itself with the resiliency of Indian rubber, but the experiment illustrates, in a simple way, what the operation hopes

to accomplish by removing the counter-pressure. Case II. proves that the expectation may be in some degree realized.

One of the questions which Cline proposed to himself before undertaking to trephine for fracture of the spine was whether a patient could recover from a compound fracture of the spine; and later writers upon the subject seem to question the possibility of it. Louis' case, however, shows that such cases are not necessarily fatal.

CASE III. (Louis' Case).^a—M. de Villedon, captain in the regiment of Vaubecourt, received a gunshot wound in the back, when turning to load his gun, at the battle at the bridge of Aménébourg. He fell on the spot, and finding himself paralysed in the lower limbs, he believed that he was mortally wounded. He did not wish to be removed from the place where he fell; but in spite of his entreaties he was conveyed to the hospital at Marbourg, where he was first attended to by M. Duplessis, Surgeon-major. Louis did not see him until the third or fourth day after the injury; M. Villedon was then in a very precarious state, suffering from high fever. It was necessary to introduce a catheter several times a day. He had been bled; the wound enlarged, and the ball extracted. Louis introduced his finger into the wound, and there felt some fragments of bone of considerable size. Louis then says:—"I consulted with my colleague M. Duplessis. He agreed with me as to the imminent danger of the patient, whom, indeed, he believed beyond the reach of aid. I recounted to him the cases of fracture of the spine which I had seen, and the causes of the fatal event which had followed. We drew a parallel as to the mode of treatment of wounds of the head, where one does not hesitate to remove fragments of the cranium which prick or compress the dura mater; we also spoke of the indication for the trephine, even without fracture, to give exit to blood, or matter, or even to prevent suppuration in some cases of fracture. I owe it to the memory of M. Duplessis to say that in this conference our way of thinking was the same.

"The wound was carefully dressed next day; and the spinal marrow having been set free from pressure, and the wound no longer irritated by the fragments of bone, an abundant suppuration became established. The patient appeared better from day to day. The lower extremities recovered their sensibility and some power of movement. Excoriations, the precursors of bed-sores, got well; in

^a Archives Générales de Médecine, série 2, Tom xi., 1836, p. 417.

fine, M. Villedon recovered. For a long time he could only move about on crutches. He made a journey to Barege; he enjoyed good health, and was at last able to walk with a stick. The lower extremities continued weak, and did not recover their original *embonpoint*, one leg being more atrophied than the other. *Quoi qu'il en soit c'est une victime que l'art a soustrait à une mort certaine."*

The cases hitherto detailed are, to say the least, calculated to encourage surgeons to follow a bolder line of practice than has been hitherto generally adopted in the treatment of injuries of the spine with pressure on the cord. When it is borne in mind that the mortality from fracture of the spine is so great as to have induced Sir Astley Cooper to say, that to save one life in a hundred would be more than he had seen accomplished by surgery; and when, at the same time, we remember that with the aid of chloroform we are justified in undertaking operations, one of the great arguments against which, in former times, was the suffering entailed by the operation itself, such considerations should embolden surgeons to give a further trial to a proceeding which is not more formidable than many of the recognized operations in surgery.

In forming a judgment on so important a question surgeons will, very rightly, be much influenced by their individual experience. Some who have been more fortunate than their fellows in meeting with cases of recovery when they had reason to believe that the spine was fractured and the cord compressed, will be opposed to the proposed operation. Those, on the other hand, who have met with none but fatal cases, and who retain a lively recollection of the dreadful course of suffering through which such cases pass on the road to death, will be more willing to have recourse to an expedient the results of which for so far are, as I say, encouraging.

In my own experience of cases of fractured spine left to run their own course, I have met with but one in which life was preserved. The case is worthy of being recorded, although not bearing exactly upon the present subject, inasmuch as I conceive that the fracture was below the termination of the spinal marrow, and consequently the symptoms arose from pressure and injury to the cauda equina, not from pressure on the cord itself.

CASE IV.—Henry Dalton, formerly a private in the 19th Hussars, twenty-six years of age, was brought under my notice in May last, by Dr. John A. Byrne, physician to the Canal-street

Dispensary. Dalton, who is a remarkably intelligent young man, gave me the following account of his case. Three years ago, when in India with his regiment, his horse, in rearing, fell upon him; as soon as he was disengaged from the weight of the horse he tried to crawl away, but found he could only attempt to do so with his hands and arms, as his legs were totally powerless; in fact he did not feel that his lower limbs were there. He was conveyed to the hospital at Cawnpore, where the surgeon told him that his spine was broken, low down. He was found to be quite paralysed both as regards sensation and motion in the lower limbs; he required the catheter to be used in order to empty the bladder, and he was unable to control the evacuations from the bowels. Apparently some attempt was made at this time to remove, by extension, the deformity of the spine which then existed; for the patient says that he was put under the influence of chloroform several times for some such purpose. His own regiment having left Cawnpore he remained there in hospital for over eight months, under the care of Dr. Meyers of the Royal Artillery, who appears to have devoted very great attention to the case. During this time he regained no power over the lower limbs, neither did sensation improve; he required the constant use of the catheter. No bed sores formed.

His present state is as follows:—

Right leg quite devoid of sensation as high as the knee; obscure feeling of contact of the hand in the thigh, in which, however, he can distinguish the difference between heat and cold distinctly; left lower limb totally devoid of feeling from the groin down.

He can raise the right lower limb by the muscles acting on the upper portion of the femur; the muscles of the leg on the same side are completely paralysed. Muscular paralysis of the left lower limb may be said to be quite complete. Muscles on both sides much wasted. No reflex phenomena can be excited in the lower limbs.

Bladder and bowels are not under the control of the will. He has learnt to introduce a catheter for himself when necessary; urine generally neutral, sometimes alkaline and cloudy with pus. Bowels generally constipated, acting about once a week. When necessary to use aperient medicine he takes sulphate of magnesia in preference to anything else. He desires, however, that the bowels shall be constipated, as he cannot control the evacuations if anything like diarrhea comes on.

He has *referred* sensations of formication in the right foot. The

temperature of feet and legs is normal; he looks well in the face, and says that his general health is on the whole good.

On examining his back (of which I have a cast in my possession) the amount of deformity taken in connexion with the history and symptoms, leaves no doubt as to fracture of the spine having taken place. The summit of the most projecting spinous process is exactly two inches above a circle made round the body, at the level of the umbilicus, so that we may presume that it is the third lumbar vertebra which has been broken.

This patient passed blood in the urine for some days after the accident, and still suffers occasionally from pain in the left kidney.

This case is certainly one of interest and importance; yet even if it was clear that in this instance the medulla itself had been injured, such cases would not be sufficient to establish a rule never to be deviated from. It might as well be argued that because instances are recorded of compound fractures of the cranium, with depression of bone, recovering without any attempt having been made to elevate the bone, in no such case should an attempt be made to elevate.

Some persons appear to think that if the spinal medulla has been for a time subject to compression it undergoes some structural change, so that even if relieved from pressure it cannot again resume its functions. Physiology has proved that this is not so; but, moreover, we see, not at all unfrequently, cases of spinal disease in which matter outside of the theca pressing on the cord produces paralysis, and after a time the purulent deposit changing its position or making its exit, the cord recovers, and the paralysis more or less completely disappears.

CASE V.—J. M'D., a woman about forty years of age, was admitted to the hospital of the Mountjoy (Female) Convict Prison on February 2nd, 1861. She complained of stiffness in her neck, and had obscure symptoms, leading to the supposition that there existed disease of the cervical vertebræ. On the night of June 23rd she became paralysed; on the following morning I saw her along with my colleague, Dr. Banon. The patient was then totally paralysed, as regards sensation and motion in both arms; in the lower limbs she could feel the contact of the hand when rubbed over the surface, and had a good perception of heat and cold, when vessels containing hot or cold water were applied to the skin; she could not draw up the legs as she lay in bed.

Reflex movements were with difficulty excited in the lower limbs, on tickling the sole of the foot, &c., but no such phenomena could be excited in the upper extremities.

She could not voluntarily evacuate either the bowels or bladder; the former were relieved by enemata, the latter by the introduction of the catheter.

She was perfectly intelligent, and her memory was unimpaired; her speech unaffected; she could swallow without difficulty, and complained only of pains in the lower limbs.

The patient continued in this state for some days. On the 2nd of July, she suddenly underwent a remarkable improvement; she found that she could draw up her legs in bed; she sat up, and fed herself with her right hand; this she could do while she looked at her hand; but if she looked away the spoon fell from her hand; she then did not know whether it was in her fingers or not. She now drew attention to a tumour, in the back of her throat, which had suddenly made its appearance, and caused her such difficulty in swallowing that she could only get down fluids. This tumour could be felt low down in the back of the pharynx.

The patient got weaker, and gradually sank, exhausted, on April 15th.

At Dr. Banon's request I made a *post mortem* examination.

The brain was healthy; and, on removing the spinal marrow, no pus or other evidence of inflammation was found within the theca. For the length of two inches the spinal cord, at the level of about the fourth and fifth cervical vertebræ, was found somewhat flattened from compression. It appeared to the touch to be softened at this point, but this was only apparent softening from mechanical pressure, for careful microscopic examination showed that the structure was not altered. A considerable quantity of purulent matter existed between the theca of the cord and the bodies of the fourth and fifth cervical vertebræ; and this collection of matter was found to communicate with the tumour felt in the pharynx, which proved to be an abscess at the back of the pharynx and œsophagus.

It appears, in this instance, that the pressure of the matter, formed behind the bodies of the vertebræ, flattened the spinal marrow, and interrupted its functions; yet, when the matter made its way forward to the back of the pharynx and œsophagus, the cord in a great degree resumed its functions; and when the patient died, a fortnight later, no structural change had taken place from which it might not have completely recovered.

In the following case also the cord seems to have recovered itself from the effects of pressure, also resulting from formation of matter outside the theca.

CASE VI.—John M'C., 26 years of age, was admitted to the hospital of the Mountjoy Prison, on July 14th, 1865, having commencing caries of the spine, in the lower part of the dorsal region. He became gradually paralysed in the lower limbs, bladder, and rectum. Later, a psoas abscess made its appearance in the left groin; it pointed, and burst. The patient recovered power over the sphincters, and by degrees was able to walk. He left hospital on October 23rd, 1865.

As showing, however, the effects of pressure from a mechanical cause on the spinal marrow, and its capability of resuming its functions when this pressure is removed, the following remarkable case is of paramount interest.

For this case I am also indebted to my friend and colleague Dr. Banon.

John Whitethorn, aged nine years, was admitted to Jervis-street Hospital on July 5th, 1862. He had been playing with some other boys, when he met with a singular accident. The game consisted in running tilt with an old umbrella at a mark upon a wall; as Whitethorn ran forward to strike the mark he was pushed from behind by another boy—one of the wires of the umbrella (one of those which crosses from the sliding ring on the handle to the ribs), being loose, entered the boy's mouth, and was driven through the left side of the soft palate, and into the spine, with force so great as to fix it in it firmly, and snap it across.

Dr. Banon happened to be in the wards at the time when the boy was brought in. He found that the wire had passed through the velum on the left of the middle line, and that it was driven backwards, probably between two of the cervical vertebræ, so as to injure the spinal cord; the wire was not broken off short, but could be seen and caught hold of in the mouth.

The boy's left arm was paralysed, as regards motion, but its sensation was unimpaired; his pulse was 75, weak, and intermittent; there was no paralysis of the bladder, bowels, or lower limbs. It required considerable force to withdraw the wire. At ten o'clock that night the pulse had risen to 112; he could move the left arm, but could not raise it off the bed.

There was fever, with delirium and sickness of stomach for some days; on the 18th the paralysis had almost disappeared—he could extend the arm perfectly from the shoulder.

Later, cerebro-spinal meningitis supervened; then came on severe pains in the neck, followed by vomiting and great restlessness. On the 15th he was roaring with pain, and could not bear to be touched. He became insensible, and died on August 18th, forty-four days after the accident.

The *post mortem* examination revealed considerable effusion at the base of brain, and evidence of meningeal inflammation around the pons, medulla oblongata, and spinal cord. I examined, along with Dr. Banon, the seat of the injury. The track of the wire was followed, as it pierced the intervertebral substance between the third and fourth vertebræ—it had evidently pushed the theca before its point—which was not sharp—and pressed it on the cord, squeezing the latter; the wire had not perforated the theca vertebralis.

Yet, from an injury such as this, the cord recovered itself so quickly when the wire was withdrawn that the same night motion began to reappear, and within a fortnight paralysis had almost totally disappeared.

There is no doubt, as is urged by those who are altogether opposed to the operation of trephining the spine, that it is a general fact that the fragments of the broken vertebræ, which commit the greatest injury to the cord, are usually in front—not behind; and this, as I have already said, seems to be the gravest of all objections which have been offered against the operation; this objection I have attempted to answer in my former communication. I would now merely add that those who put it forward take little or no account of the cases of fracture of the spine, in which the posterior part of the vertebra is broken, and the spinal marrow pressed upon by a portion of bone actually capable of removal. Although such cases may form but a small proportion of the whole number of fractures of the spinal column, yet the records of surgery show that they are, at least, too frequent to be passed over in silence; they are obviously the cases most favourable for the so-called operation of trephining; and a perusal of some of those, which I have here collected from various journals, will, I think, show that among the recorded cases of fractures of the vertebræ there are a good many in which it would have been good surgery to have performed the operation in question.

The following cases are intended merely to illustrate the fact that the posterior portions of the vertebræ may be broken, yet the bodies escape fracture or displacement more frequently than some persons would have us believe.

CASE VII.^a—On the 1st of May, 1851, during a violent storm of wind and rain, a balustrade fell from the top of a high building, striking a man, named John Larkin, who was about forty years of age, upon the back of his head and neck. He fell to the ground instantly, and did not again move his feet or legs, although he never lost his consciousness, until he died. I found his bladder paralysed also, and his left arm; but his right arm he could move pretty well. He conversed freely up to the last moment, and said that he was suffering a good deal of pain, which was always greatly aggravated by moving. His death took place 36 hours after the receipt of the injury.

Dr. Hugh B. Vandeventer, who was the attending surgeon, made a dissection on the following day in my presence, which disclosed the fact that the plates of the sixth cervical vertebra were broken upon each side, and that the spinous process, with a small portion of the arch attached, was forced in upon the spinal marrow. There was no blood effused, or serum at this point, but about one ounce of serum was found in the cavity of the tunica arachnoides, at the base of the brain. The bodies of the vertebræ were not broken. It was our opinion, therefore, that the immediate cause of his death was the direct pressure of the spinous process.

CASE VIII.^b—A somewhat similar case to the foregoing is recorded by C. J. Weigel. The patient, sixty-eight years of age, fell from a tree and sustained several injuries. He had complete paralysis of the lower limbs immediately after the fall, but the arms did not become paralysed until the following day. Swallowing was attended with effort, and respiration laborious. He died on the second day. The spinous process of the sixth cervical vertebra, with a piece of the arch on each side, was found to be fractured.

CASE IX.^c—W. J. Thomas, of Liverpool, has reported the case of a woman, forty-one years of age, who met with a fracture of the

^a Hamilton—*Fractures and Dislocations*, p. 150.

^b Weigel and Ammon's *Monatschrift*, Bd. i., 1838, p. 633.

^c *Monthly Archives of Medicine*, Vol. i., p. 408.

spine in consequence of falling down a flight of stairs while intoxicated. She died on the fourth day after the accident.

“The arch of the fourth cervical vertebra was fractured at the base of the spinous process. The wing of the right arch was tilted under the spinous process of the third vertebra, and there firmly wedged.”

There was no fracture of the bodies, but a separation to some extent, between those of the fourth and fifth cervical vertebræ. On opening the membranes there was no effusion, nor were there any marks of inflammation.

CASE X.^a—George Jones, aged thirty-two, a large muscular man, was admitted into Rahere's Ward, St. Bartholomew's Hospital, on the night of the 13th June, 1830, with complete paralysis, and loss of sensation of his lower limbs and trunk, as high as the middle of his sternum; his pulse was 76 and soft, countenance calm, intellect perfect, temperature of the lower extremities natural, partial priapism, respiration performed wholly by the diaphragm, but without difficulty. The previous afternoon, whilst driving rapidly a light cart with two men in it, and endeavouring to restrain the horse, the reins broke, and he fell towards the ground, the wheel passing over the back part of his neck; he was stunned, and on recovering found the lower part of his body paralysed. The patient died ten days after the injury. The arch of the sixth cervical vertebra was found fractured. There was slight separation between the seventh and eighth vertebræ. The external appearance of the cord was natural; but on section the medullary matter was found softened. The viscera of the thorax and abdomen were natural.

CASE XI.^b—J. S., aged 30 years, was injured August 18th, 1850, under the following circumstances. Being engaged in a playful scuffle, he seized his antagonist by the leg, and was in the act of pushing him over backwards; and while in a stooping posture he received a sudden twitch which threw him forwards with considerable violence, the head being flexed upon the chest in such a manner that he struck upon it, and then keeled over. All present agreed that he could not have received any direct blow upon the neck, as the ground was quite smooth. He immediately found

^a Stafford—*Treatise on the Spine*, p. 102.

^b Ladd.—*Boston Medical and Surgical Journal*, 1852.

himself perfectly helpless. I saw him about three hours after the injury; there was complete paralysis and anesthesia of the body and limbs. He complained of pain through the lower part of the cervical region, extending to the top of the shoulders. There was extreme pain on pressure, but no crepitus or deformity could be detected. On the following morning febrile action set in briskly. It was necessary to introduce a catheter to relieve the bladder, and the bowels acted involuntarily. In a few days the urine became alkaline and loaded with mucus; afterwards purulent, bloody, and very offensive. During the first forty-eight hours he regained the power of slight voluntary motion in the forearm, after which there was no improvement whatever in motion or sensation. In about ten days the fever, for the most part, subsided; the tongue cleaned, and appetite returned, but his flesh wasted rapidly; in fact the vital powers seemed to be slowly but steadily giving way. He now began to get paroxysms of dyspnea, and these became more frequent, until he sank at last, exhausted, thirty-six and a half days after receiving the injury.

Post mortem examination ten hours after death.—The six inferior cervical vertebræ were removed entire, when it was found that the spinous process of the fifth was fractured through the laminæ, and pressed down upon the cord. The fragment was with some difficulty removed, in consequence of the inequalities of the broken surfaces. The cord appeared much flattened, the membranes were entire; on opening the theca the cord was found to be of the consistence of cream at the seat of injury; below this slightly softened.

CASE XII.^a—Joseph Feugré fractured his spine by a fall on his back from a height of about fifteen feet. Five days after the accident he was admitted to the Hospice de la Magdeleine at Rouen. The lower limbs, bladder, and rectum were paralysed.

He died fourteen days after the accident. A portion of the spinous process of the third dorsal vertebra was detached; a fissure passed through the laminæ of the fourth; a considerable effusion of blood occupied the spinal canal.

CASE XIII.^b—Patrick Gallagher, collier, aged thirty-seven, while at work in a coal pit a large stone fell from the roof, struck

^a *Histoire de quelques affections de la colonne vertébrale*, par Alexandre Demussy, p. 6.

^b *London Medical Gazette*. Vol. ii., for Sessions 1843-4, p. 359.

him on the back, and crushed him. Entire loss of motion and sensation below the seat of injury; retention of urine.

Death seventeen days after the accident.

“Upon removing and sawing the injured part of the spine through the centre from back to front, the intervertebral cartilages between the eighth and ninth dorsal vertebræ were found wanting, having been destroyed by absorption or ulceration. The surfaces of the same vertebræ were bare, soft, and soaked in pus. There was some extravasation between the membranes within the sheath of the cord, and the latter was much compressed and lacerated by a small, thin, pointed fragment of bone, which had been detached from below the base of the spinous process, and driven inwards.”

CASE XIV.^a—M. Chappillon met with a case in which a bookseller, M. Duchesne, was injured by a pile of books falling upon him from a height of twenty feet. The patient died sixteen hours after the accident. The spinous processes of the second, third, fourth, and fifth vertebræ were found to be fractured and separated from their bodies; but these latter were not displaced; the fibro-cartilages uniting them only, appeared to be more relaxed and movable than they usually are.

CASE XV.^b—Ekstroemer, of Stockholm, has recorded a case of a miller who received a severe injury of the spine in the cervical region by falling headlong from his mill; he had paralysis of the extremities, and of the sphincters, &c., with diaphragmatic respiration.

The spinous processes of the fifth, sixth, and seventh cervical vertebræ were found to be broken, and pressed in upon the medulla.

CASE XVI.^c—Mr. Belcher, of Burton-on-Trent, reports the case of a man, thirty-seven years of age, a sawyer, who fractured his spine by falling backwards from a cart on which he was standing, so that the vertex seemed first to strike the ground.

^a Archives Generales de Médecine, 1836. Tome xi., p. 421. M. Louis, who reports this case in this very remarkable memoir, read to the Academy of Surgery on April 18th, 1774, makes the following observations on it:—“Cette blessure a dû paraître audessus des ressources de l’art; cependant dans un accident aussi fâcheux et tout-à-fait désespéré que risquerait-on à pratiquer une incision par laquelle on pénétrerait jusqu’aux apophyses fracturées, pour débarrasser la moelle épinière des esquilles qui la blessent, et donner issue au sang épanché?”

^b Zeitschrift für d. gesamt Medicin. (Hamburger.) Band xiii., 1840, p. 408.

^c British Medical Journal, 1862, Vol. ii., p. 531.

He died on the fifteenth day.

The spinous process of the first dorsal vertebra was splintered, and a portion detached. The articular processes of the first and second dorsal vertebræ were fractured, and their capsular ligaments torn; the arches of the vertebræ separated to some extent. There was no dislocation or fracture of the bodies of the vertebræ.

CASE XVII.^a—Schallenmüller also gives a case in which the spinous process of the sixth cervical vertebra and the laminæ on each side, at their union with the transverse processes, were broken, the body escaping without fracture or displacement.

CASE XVIII.^b—Louis mentions a case in which the spinous and transverse processes of the sixth and seventh cervical vertebræ were broken without fracture or dislocation of the bodies.

CASE XIX.^c—Boyer narrates the following—"A sack of flour weighing three hundred pounds, fell on the nape of the patient's neck; an acute pain was felt in the lower part of the cervical region. The patient was conveyed to the Hospital of La Charité. On examining him I perceived that the spinous process of the seventh cervical vertebra was more prominent than in the natural condition. The upper and lower limbs were paralysed; respiration laborious; the rectum and bladder incapable of acting. The patient died at the end of five days. At the *post mortem* examination we found a fracture of the posterior arch of the seventh cervical vertebra, with depression of a fragment which pressed on the spinal marrow, and exercised upon it firm compression."

CASE XX.^d—Professor Weber, of Bonn, records the following case.

A patient, forty-six years of age, was precipitated down a staircase, with a sack on his shoulders, and was immediately brought into the "Klinik," paralysed. Between the fourth and sixth vertebræ of the neck there was to be felt a hollow which could not be removed by cautious stretching of the neck. There was complete paralysis both of sensation and motion of the lower limbs, the

^a Medic. Correspondenzblatt (Wurtemberg), Band 13, 1843, p. 364.

^b Archives Generales de Med., 1836, Vol. xi., p. 412, Case v.

^c Traité des Maladies Chirurgicales. Third Edition. Vol. iii., p. 137.

^d Chirurg. Erfahr. und Untersuchungen. Berlin, 1859. Page 164.

bladder and rectum. The urine was drawn off by the aid of the catheter. On the following day the upper extremities also were paralysed, and the patient died fifty hours after the accident. The arch of the fifth cervical vertebra was broken and dislocated so as to compress considerably the spinal marrow. Some inflammation of the membranes extended upwards to the cranium.

CASE XXI.^a—Mulder mentions the case of a man aged thirty-eight years, who, from a fall backwards on the ground, fractured the spinous processes of the fourth, fifth, and sixth cervical vertebræ. The bodies were not broken, but the posterior arches were broken into several fragments. The patient lived two days.

CASE XXII.^b—Jaeger reports a case in which the spinous process of the sixth cervical vertebra was fractured by a waggon laden with wood passing over a man's neck. There was no trace of injury done to the skin, but there was great tenderness over the spinous process of the sixth vertebra; the lower limbs were paralysed, and some hours later the arms also. The patient died, convulsed, in eighteen hours. The medulla was torn beneath the fractured portion of the vertebra, and blood was effused within the theca.

CASE XXIII.^c—Mr. Arnott exhibited to the Pathological Society of London a specimen taken from a patient, aged seventy-four years, who died in University College Hospital, on July 30th, 1850. In this case the spinous process of the axis was found broken off, with a portion of its laminae. The broken off spinous process was much depressed below the others, and wedged in between the axis and the vertebra below.

CASE XXIV.^d—This case, reported by MM. Maunoury and

^a R. Stroemer, Ueber d. Bruch der Wirbelbeinforsätze. Würzburg, 1842. P. 12.

^b Hedinger, Ueber d. Knochenverletzungen, &c. Leipzig, 1833. Page 90.

^c Lancet, 1850. Vol. ii., p. 588.

^d Gazette Medicale de Paris, 1842, p. 361.—The reporters of this case add to it the following observation :—"In Bressant's case there was only a fracture of the vertebral lamina, effusion of blood between the dura mater and the bone, integrity of the medulla. "Dans ces cas, si on pouvait pressentir le peu d'étendue de la lésion, que n'aurait on le courage d'aller à la recherche de la fracture, de détacher la lame osseuse, qui comprime la moelle et qui est la cause infallible d'une mort rapide; il a fallu certainement plus de hardiesse pour appliquer plusieurs couronnes de trépan sur le crâne, pour faire le ligature des artères iliaques primitives, et de l'aorte abdominale, pour enlever la matrice en totalité."

Thore, and was one of those met with in the practice of Professor Roux, in the Surgical Clinique of the Hôtel Dieu, in 1841.

Bressant, forty-eight years of age, was brought to the Hôtel Dieu immediately after a fall upon his back from a height of about twelve or fourteen feet. He was stunned, but soon regained consciousness. On admission there was paralysis of sensation and motion, the skin was insensible as far up as the base of the thorax, following a line corresponding with the vault of the diaphragm. The muscles of the forearm were paralysed, respiration diaphragmatic, respiratory murmur normal, pulse quiet, paralysis of the bladder, priapism of which the patient was not conscious.

Death on the sixth day.

There was fracture of the laminae of the fifth cervical vertebra, the spinous process of which was removed with facility. Beneath the fracture there was some black blood between the dura mater and the bone. The medulla was uninjured. There was also fracture of the sternum or separation between the manubrium and body of that bone.

CASE XXV.*—A Negro, aged thirty, while wrestling with a companion, was thrown suddenly upon his neck, by having his feet tripped from the ground. The fall was immediately succeeded by a loss of motion and feeling in the shoulders and arms, in the walls of the chest and abdomen, and in the lower extremities. Though there was an entire loss of sensibility to the impression of external agents, he was subject to occasional and severe pains in all the paralysed parts, and to constant and lancinating pains in his arms and shoulders. There was no external mark of injury over the spinous process of the fifth cervical vertebra; but a distinct crepitus was perceptible on pressure. His breathing was short and extremely laborious, being carried on alone by the action of the diaphragm. The muscles of the head and neck, above the origin of the phrenic nerve, maintained their integrity. His pulse and the temperature of his body were unaffected until near the close of life, which occurred within forty-eight hours after the accident. On the day subsequent to the injury he was affected with retention of urine and great abdominal distention; notwithstanding the peristaltic action of the bowels had been excited, and met with no resistance from the paralysed sphincter. There was no distention of the corpora cavernosa.

* American Journal of Medical Science, 1837, Vol. xxi., p. 276.

Dissection.—The rim or arch of the fifth cervical vertebra was fractured in three places, and the spinous process, with a part of the arch, was driven in upon the spinal marrow. There was a slight effusion of blood between the sheath of the spinal marrow and the bone, and a considerable effusion between it and the substance of the cord. There was no material lesion of the medulla or of its investing membranes; and the body of the bone was not fractured or displaced at the intervertebral junction.

CASE XXVI.*—A. B., a mason, twenty years of age, while going up a staircase with a hod on his shoulder, made a false step and fell backwards; in the fall the posterior part of the cervical region of the spine struck with violence against the corner of one of the steps.

When carried to the Hôtel Dieu the patient presented the following symptoms:—Paralysis of the limbs; insensibility of the skin, which was cold (especially that of the extremities). His respiration resembled that of a man in a troubled sleep; the pulse was very slow (40 to 50 in the minute); slight drowsiness; voice feeble; he was slow in answering questions addressed to him; his eyelids drooped; there was erection of the penis. The walls of the thorax were immovable during respiration which was performed by the diaphragm.

On the day following the accident deglutition was very difficult; respiration more laboured, especially in the evening; every instant the patient was threatened with suffocation. The priapism was persistent.

A catheter was introduced to draw off the urine, without his perceiving it in the least. Dejections passed involuntarily; pulse small, very slow. All the parts below the lesion of the neck were completely paralysed. The patient understood everything, answered correctly what he was asked, and only complained of the painful feeling of suffocation.

The *autopsy* revealed a fracture, with depression, of the laminae of the fifth cervical vertebra; the spinal marrow was firmly compressed at the point corresponding, and was reduced to one half its ordinary volume; the substance of the medulla was marked with striæ of blood.

These cases show, if nothing else, at least that instances of fracture of portions of the vertebræ, unaccompanied by fracture or

* Ollivier. *Traité des Maladies de la Moelle Epinière* Third Edition. Paris, 1837. Vol. i., p. 270. Case 9.

displacement of the body, are not very unfrequent; in fact, I candidly confess, that until I had looked into the published details more carefully than I had hitherto thought of doing, I accepted the assertions of others, and was not prepared to find so many. Those which I have here brought together represent rather more than ten per cent. of the recorded cases upon which I have been able to lay my hands. In the great majority of such cases the fracture occurs in the cervical or upper part of the dorsal region; the reason of which is obvious both from the conformation of the spine and the exposure of these regions to injuries by direct violence. In discussing the question as to whether the operation of trephining the spine is to be admitted among the legitimate and recognized operations in surgery, injuries of the kind above mentioned have a considerable importance. They prove that after all, among the cases of fracture of the spinal column, a goodly per centage are of such a nature that even the sternest adversaries of the operation would admit them not to be unsuitable for it. The question here only turns upon the difficulty of diagnosis.

The real question as to the fitness or unfitness of any case for operation will, I conceive, depend far more upon the exact amount of injury done to the spinal cord than on whether the fracture engages the portions of the vertebra in front of it or behind. No doubt the operation would have a good chance of being brilliantly successful in such cases as Case VII., XI., XIX., or XX.; but assuredly there are not a few cases in which, although pressure may arise from injury done to the vertebra anteriorly, yet removal of the counter-pressure posteriorly will set the cord free from being squeezed. When the body of a vertebra is broken, and, as usually happens, the intervertebral substance is at the same time more or less lacerated, the progress of the case must be, under the most favourable conceivable circumstances, very tedious. The first thing that nature does is to absorb the lacerated fibro-cartilage, and while this is being accomplished little or no callus is thrown out. I find, however, a considerable number of cases reported in which the body of a vertebra is so injured as to press upon the marrow, although no separation of it into fragments has taken place; neither are the fibro-cartilages torn. The accompanying woodcut, Fig. 2, shows the appearance in a case of this kind mentioned by Professor Middeldorpf. The body was flattened by compression so as to bulge backwards into the spinal canal. The spinous process of the vertebra above was also broken.

Fig. 2.

The most remarkable case of this kind that I have seen is one which was communicated to the Surgical Society of Ireland, last March, by Professor Geoghegan,* by whose kind permission I

Fig. 3.

Fig. 3, showing compression of one of the dorsal vertebrae. The bodies of the vertebrae are sawn vertically through the centre, and the section viewed from the anterior aspect, in order to show the exact character of the lesion.

* Medical Press and Circular, March 21st, 1866, p. 277.

obtained a woodcut from the preparation. The patient fell into an area on his head; by the indirect violence thus communicated to the spine the fifth dorsal vertebra was compressed in the manner exhibited in the woodcut, Fig. 3. It was in fact flattened so that it bulged backwards, encroaching on the spinal canal. No portion of the vertebra was actually detached, neither was the fibro-cartilage torn either above or below the compressed body.

In injuries of this kind if, by taking away the posterior arch or arches, the pressure on the cord is relieved so as to enable the patient to escape from the immediate risks to life, there may be reason to hope that the patient may ultimately recover, as do cases of curvature of the spine from caries, where the deformity is as great or even greater.

When, in consequence of a fracture of the spine, the marrow is subject to pressure—but not lacerated—it is usual, unless the pressure be so considerable as to destroy both sensibility and motion below the injury, to find that the functions relating to motion are much more affected than those of sensation; the patient cannot attempt to move his toes or feet; cannot draw up his legs in bed, or even push them down if flexed for him, while feeling may be normal even to the tips of the toes; the sphincters may be paralysed, yet the patient distinguishes the temperature of an enema, and complains on the introduction of a catheter; if the spinal marrow be lacerated to any considerable extent, or what is equivalent to laceration, if it has been so roughly compressed as to disintegrate its structure, the case is different—sensation and motor power are then alike lost.

Between these two supposed conditions there are many degrees; and an accurate examination of the symptoms connected with sensation must be the chief aid in making an accurate diagnosis. The amount of sensibility on one side as compared with the other, and estimated by application of the points of a pair of compasses—the appreciation of the sensation of heat and of cold—of contact, of tickling, and of the position of the limbs—should each be looked to. The amount of voluntary power affecting certain muscles will assist in determining the seat of pressure; and if paralysis of motion is found on one side in a degree much more marked than on the other, while sensibility is observed to be greatest in the part where motor power is least, there need no longer be any doubt that the pressure is chiefly exercised on the lateral half of the cord on that side of the body on which the motor paralysis is found to be greatest.

I have lately seen, with Dr. Darby, of Bray, a case in which the pressure caused by the formation of matter strikingly illustrated this fact.

I am indebted to Dr. Darby for the notes of the case.

CASE.—Pat Byrne, aged eight years, healthy, active, and intelligent, for some months in the school of the Rathdown Workhouse, was brought under Dr. Darby's notice on the 9th of March, 1866, by his father, who said he had observed the boy for some days past holding his head awry; the boy, on being questioned, said he did not know what had brought it on, and added that he had no great pain—he did not mind it; his head was inclined to the right side, and could not be turned straight; in other respects he appeared healthy; he was ordered to hospital, and directed to take a purgative, and have a warm bath; the head continued awry, but his health continued good; he remained in hospital, and had poultices, leeches, and such care in medical treatment (purgatives, &c.), as the case seemed to require, up to the 25th, when he first complained of want of power, and pain in his right arm. On this day his pulse was quick—tongue clean—appetite good; leeches were applied, and such other treatment as appeared necessary.

On the 10th April his right arm was quite paralyzed, and he showed some febrile symptoms, and a fulness was observed in the right side of the back of the neck.

On the following day I saw the boy along with Dr. Darby. The patient could not make the faintest attempt at moving his right arm, or stir any of the fingers; he could move the left arm freely about, and grasp tolerably firmly with the hand; as to feeling—the symptoms were reversed; in the left hand he could hardly feel at all, so that when the tops of the fingers were tightly pinched he did not wince, while the slightest pressure on the fingers of the right caused him great distress; at this time both sensation and motion in the lower limbs were unimpaired. The opinion formed was, that a deep-seated abscess, possibly connected with diseased bone, was pressing on the right side of the spinal cord in the cervical region. He became worse, and died on May 4th.

An abscess was found to exist, pressing, as had been supposed, on the cord; this collection was partly external to the spinal canal—partly within it; the pus escaped when the vertebræ were being removed; from an examination of the cavity it appeared that, external to the theca, and between it and the bone on the right side,

there had been a collection of matter of about the size of a small filbert; this communicated with a larger cavity external to the canal, situated anterior to the transverse processes, and very deep among the muscles of this region.

In seeking to excite reflex movements in the lower limbs of patients suffering from fractured spine the observer is often disappointed in being able to do so, and this too, when the injury is so high up as to lead one to suppose that the lower portion of the cord is intact. Some persons, for whose opinions I entertain the highest respect, regard the absence of the capability of exciting reflex movements in such cases as a proof that the injury is not restricted to a local compression of the cord, "but that it involves also such a molecular disturbance of its structure as appears to be propagated throughout the entire segment of the organ inferior to the direct seat of mischief." Were this view correct, it would appear that a total absence of reflex movements in the lower limbs would indicate an extensive lesion of the medulla, not possible to be removed by getting rid of all pressure at the seat of injury. As I believe, however, that there is another and very different explanation to be offered concerning this symptom (or rather absence of a symptom), I may be pardoned for stating what I conceive to be the true physiological solution of the problem.

When a patient is suffering from inflammatory softening of the spinal cord in the middle of the dorsal region, or higher up, energetic reflex movements can always be excited in the lower limbs by tickling the soles of the feet, plucking the hairs on the inside of the thigh, and so forth. In short, when by disease the lower portion of the spinal marrow is cut off more or less completely from communication with the sensorium the portion so cut off retains its power of giving rise to energetic movements when excited, so as to call its reflex action into play.

What is then the reason that this does not also happen when, by a fractured or displaced vertebra, or pressure from any cause, the lower portion of the medulla is cut off from communication with the sensorium. It must be remembered that in cases of paraplegia dependent on congestion of the spinal cord or its meninges, it is usually difficult and very often impossible to excite reflex movements, and that this constitutes one of the most important diagnostic marks of this form of disease. If, therefore, any mechanical cause gives rise to congestion of the cord, it is reasonable to suppose that a similar state of things will be found to exist.

It is therefore simply venous congestion of the cord, resulting from pressure at the seat of injury, and not a molecular change pervading its whole structure, that causes the possibility of exciting reflex motions to be in abeyance. If this view be correct, so far from being a reason against the operation, the absence of reflex movements is an additional indication of the necessity of, so far as possible, getting rid of mechanical pressure on the cord.

In support of the accuracy of this view it may be mentioned that in injuries affecting the lower portion of the cord it may be found impossible to excite reflex movements in the feet, toes, or muscles of the calf, while stimulation of nerves, whose roots enter the cord above the seat of injury will call forth energetic movements in the sartorius, cremaster, &c.; moreover, if these movements are excessively active it may, along with hyperesthesia of the surface, be taken as evidence that the cord immediately above the injured part is in a state of vascular excitement, tending to inflammation of either the cord or its membranes.

So far as is at present known, the occurrence of priapism does not appear to yield any special information, either as to the precise portion of the marrow injured or the amount of pressure exercised on the cord. It seems to give evidence of excitation of the upper part of the cord, and nothing more. Priapism is mentioned as having taken place in about thirty per cent. of the cases collected by Gurlt, in which the injury occurred above the second dorsal vertebra; when the injury to the cord is lower down it is of much rarer occurrence; and even then, I suspect, is really produced by an excitation of the marrow above the injured part. It is a symptom which is usually transient, seldom lasting more than a day or two, although in some cases persisting for a fortnight, or even longer.

The remarkable increase of temperature noticed in some cases as occurring in consequence of injury of the spinal cord, does not any more than the occurrence of priapism, appear to indicate the exact part injured, or the amount, or nature of the injury. Indeed, it is greatly to be regretted that the cases in which this phenomenon has been noticed have not been more carefully investigated. Sir B. Brodie mentions a case in which the thermometer rose to 111° F., when the bulb was placed between the scrotum and the thigh; he does not state what the temperature was above the injured part. Mr. Butcher, in the account given of his case of dislocation of the cervical vertebræ, without fracture, states that the heat in the

lower limbs was absolutely greater than natural. What the temperature was either above the injury or in the lower limbs, is, unfortunately, not stated. To have proved that the blood circulating in the paralyzed parts was absolutely so many degrees warmer than that in the parts above the injury would have been most interesting; it is also to be regretted that among the symptoms noted concerning the same important case no mention is made of the capability of exciting reflex movements in the parts below the seat of injury.

The examination of the spinal column itself at that part where the lesion has taken place may naturally be expected to give much information as to the degree of benefit which may be hoped for from the operation of trephining. The amount of deformity, the degree of tenderness, and general tumefaction—the possibility of detecting crepitation or displacement of the spinous processes will each indicate something in favour of the operation, or the reverse, according to circumstances. The nature of the injury and the exact mode in which the accident happened should be carefully inquired into. It is only necessary to read some of the cases already quoted to see that the close investigation of the precise circumstances of the accident may yield evidence which, taken in connexion with existing symptoms, will aid more than anything else in forming an accurate diagnosis as to the exact nature of the lesion.—(See cases VII. and XXVI.)

If, before the operation be undertaken, there is evidence that myelitis or meningitis has already supervened, it is obvious that little benefit could be expected from the performance of it. If, therefore, the patient complains of the well-known symptoms of feeling as if a cord were tied round the body at the limit of the paralysis, and formication, and “*pins and needles*,” not only in the parts still sensitive, but where sensation has been already lost; if there are cramps and spasmodic twitchings in the feet, calves, or abdominal muscles, and if, at the same time, there is an accession of fever, quick pulse, &c., accompanied with excessive sensibility of some portions of the surface, operation need not be contemplated; if it is to be done with a reasonable prospect of success, it must also be undertaken before those changes have taken place which give rise to atrophy of the muscles, bed sores, alkaline, and muco-purulent urine, &c.

There appears to be nothing to contraindicate the free use of chloroform during the performance of the operation. It was administered so as to produce its full effect in the case operated on

by myself, in Dr. Gordon's case, and in a third case in which my colleague, Dr. Henry J. Tyrrell, lately performed this operation in Jervis-street Hospital. In all these cases the injury to the spine was low down; although the patients were turned over so as to lie on the front of the body, there was no difficulty experienced in giving it. In Jones's case, where the injury was high up in the dorsal region, and the action of the heart was intermittent, the patient was nevertheless placed under the influence of chloroform, on two occasions, without bad result—once for the purpose of minute examination, and a second time for the operation.

A very considerable number of the recorded cases of fractures of the spinal column show that after death softening of the medulla has been observed at the seat of injury. My own observations, as well as those of others, have convinced me that in many instances this softening is something different from inflammatory softening on the one hand, and white or atrophic softening on the other. In both, inflammatory and ordinary white softening, it is probable that the nervous tissue has undergone a structural change from which it cannot recover. In that kind of spurious or mechanical softening which the cord undergoes from the pressure of purulent matter, bone, &c., it is quite certain that it can recover; it has undergone no structural change which prevents it, as soon as the removal of pressure permits the circulating fluid to permeate it, and nourish it once more, from recovering itself and resuming its functions. It probably best bears comparison with that kind of atrophic softening which may result from embolism, and which may, if the collateral circulation be so re-established as to restore nutrition to the part, be followed by recovery.

In Cases II. and V. I examined the structure of the medulla with great care, but without finding either exudation granules, or traces of fatty degeneration. The medullary substance was bloodless from pressure, and nothing more; when the finger was passed down gently along the cord, at the part which had been compressed, it felt softer; and when moved between the fingers it felt more limp at that part, yet the microscope gave no evidence that any structural change had taken place. We must not, therefore, suppose that in all the published cases in which softening is said to have occurred that myelitis had taken place.

I have now seen the operation performed three times on patients, and I have performed it many times on the dead subject. The mode of its performance, and the difficulties which are encountered

in accomplishing it, vary much in the different regions of the spine, and, of course, in actual practice must vary with the nature of the case and the mode of injury. When the spinous process and laminae only are broken the whole proceeding will be very simple; but even when this is not the case it may be said, that compared with many of the great operations in surgery, that of trephining the spine is not so formidable as has been supposed.

In the lumbar region and lowest part of the dorsal the operation offers the greatest difficulties. The patient should be placed face downwards, with the feet towards the window, so that the light may fall into the bottom of the wound. The body should be placed as evenly and flat as possible on its front aspect; but the person who gives chloroform can keep the head turned a little on one side, and somewhat raised, by placing one of his hands under the chin. An incision from four to five, or six, inches long is to be made through the integuments over the spines, with a strong scalpel. A strong curved bistoury is the best instrument with which to divide the muscles on each side of the spinous processes; and this can be done, at once, more rapidly, and closer to the bone, by entering the point of the bistoury below and cutting upwards and backwards. As soon as the mass of muscles on each side are to a sufficient extent divided, the operator should fix, himself, on each side, a retractor such as is here represented, Fig. 4, and give each in charge to an assistant.

Fig. 4.

The broad flange of such a retractor not only keeps the wound well open, so that the operator can see what he is about, but by pressure prevents hemorrhage, while the hand of the assistant who holds it does not get fatigued, owing to the other end being bent. The operator should next feel in the sulcus, on each side of the spinous process, for the inequality caused by the injury, and by grasping successively each spinous process, in a pair of necrosis forceps, ascertain whether the posterior portions of any vertebra are broken. He must then determine which laminae are first to be divided; and before doing this it is well to place sponges, wet with iced water, in the hollows on each side of the spinous processes, and give time for all hemorrhage to cease.

Having singled out the spinous process of the vertebra, the laminæ of which are to be divided, the interspinous ligaments above and below it are to be cut, and a part of the process taken off, with bone forceps, leaving, however, enough to grasp with the necrosis forceps, when it becomes necessary to remove that portion, after division of the laminæ. The division of the laminæ, in this region, is exceedingly difficult. The trephine cannot be used; and Hey's saw, owing to the depth of the wound, and oblique manner in which the bone must be sawn, is also useless. The division is best accomplished by strong forceps bent at an angle, something like the forceps known as "Fergusson's side-cutting forceps." Experience shows that the laminæ may be thus cut through without fear of injuring the cord or its membranes. As soon as this has been accomplished, the portion of the spinous process still remaining is to be caught in a pair of necrosis forceps, and the portion of bone raised and removed by carefully dividing the ligamentous structures still holding it. When the posterior arch of one vertebra has been taken away, the removal of the second is comparatively easy; the best method of effecting this is by nibbling it away bit by bit, with the ordinary gouge forceps.

It is a much easier matter to remove a spinous process, and the laminæ on each side of it, in the middle or upper part of the dorsal region than in the loins. In its preliminary stages the operation is like the foregoing. When the muscles on each have been divided and retracted, as already described, owing to the mode in which the spinous processes of the dorsal vertebræ are imbricated one over the other, it becomes necessary to remove the spine above that which, with its laminæ, is to be first taken away. This having been done the spinous process of the vertebra, the laminæ of which are to be divided, should be taken away, as close to its root as possible, with gouge forceps. With a little care the spinous process can be so completely taken off as to leave the surface sufficiently flat for the application of the crown of a trephine, which should in general be about one inch in diameter, and furnished with a strong centre pin, a little longer than usual. As the bone is of a much softer texture than that of the cranium, the trephine once planted, works rapidly, so that after a few turns the centre pin should be withdrawn, and it should then be worked gently until it is found possible to move, and subsequently elevate, the trephined portion; after this is taken out it will be seen that in the dorsal region there is a sort of provision which enables one to use the

trephine without much danger of its, in any way, injuring the cord. In fact the articulating processes of the vertebra, next below, look backwards in such a way as to prevent the instrument from going suddenly in so as to injure the medulla. The posterior arch of a second vertebra may now be easily taken away, either by a second application of the trephine, or better still, by the aid, simply, of the gouge forceps.

In the cervical region the removal of the bone can be effected with great facility by the gouge forceps alone; here, as well as in operating on the first dorsal vertebra, the trephine cannot safely be employed. After the skin has been divided and the muscles detached on each side the interspinous ligaments are to be cautiously cut, taking care not to sink the scalpel too deeply; the spinous process is next taken off with the gouge forceps, and at once the ligamentum subflavum comes into view, and the vertebral canal is opened to a small extent. Bit by bit the posterior arch is then to be taken away by the gouge forceps, until enough is removed. The removal of a second or third can be even more readily accomplished with the same instrument. The retractors employed for holding back the muscles on the neck should not be so broad as those intended for the operation on the back or loins, and the lower edge of the retracting flange should be somewhat convex.

In all the cases which I have seen the wounds have gone on favourably under the simplest treatment; water dressing at first; and after suppuration has become established, charpie, with simple dressing, or a poultice with some "warm dressing" under it.

It is unnecessary to say that every possible attention must be paid to the general health of the patient. He should be placed on a water bed, the bowels and bladder carefully attended to, and any tendency to bed-sores guarded against with the utmost anxiety. For this purpose he must be shifted from side to side, and occasionally placed upon his back, even at the risk of pain in the wound or disturbance of the broken vertebra. To avoid bed-sores and keep the bladder in good order must be the first object of the surgeon, and no entreaties or complaints should influence him in deferring to do what he considers necessary for this purpose. The bed clothes must be frequently changed, and kept perfectly dry. If any bed-sores have already formed, every effort should be made to get them cured; if the urine is already alkaline and mucopurulent the bladder should be washed out every day, or several times each day.

Immediately after the operation the object of medical treatment should be to avoid inflammation of the cord or its membranes, and with this view belladonna or atropia should be administered, with the intention of keeping the medulla as much as possible in a condition of repose; opium should not be given; but if urgently demanded to allay pain it should be joined with belladonna, in doses proportionally large. Somewhat later iodide of potassium may be ordered, and when all apprehension of inflammation is past, strychnine. Indeed, it will be in different cases a very nice and at the same time very important question to determine whether it may not be good practice to give strychnine early, even at the risk of calling forth inflammatory action in the cord. Of course, its administration would not be contemplated if any tendency to myelitis can be detected. But if pressure on the medulla for a longer or shorter time has given rise to that kind of mechanical softening of it which, without being identical with white or atrophic softening, is closely akin to it, and if it seems that the parts below are not recovering their vital powers after the pressure has been got rid of, it may become necessary to make a desperate effort to rouse the dormant energies of this portion of the marrow, regarding the chance of exciting inflammation of it as the less great danger of the two. Under these circumstances I should not hesitate to give strychnine cautiously before many days after the operation.

Shampooing of the limbs and the use of electricity should be early applied, in order to arrest the atrophy of the muscles. Whatever doubt may exist as to giving strychnine at an early period after the operation there need be none about its administration after the risk of inflammation is past; it should then be added to good nourishing diet, with wine, beer, and iron tonics.

I have alluded in the foregoing pages to a case in which my colleague, Dr. Henry J. Tyrrell, lately trephined the spine, in Jervis-street Hospital. The case will, no doubt, be more fully reported hereafter. I may mention, however, that the patient, a young man twenty-one years of age, met with the injury of the spine three weeks ago; he was operated on forty-eight hours after the accident. During the operation he was thoroughly under the influence of chloroform, and since the operation the wound has gone on admirably. Whether this case may prove successful or not, it cannot be said that the patient's suffering has been materially

increased, or his duration of life in any way shortened, by the operative procedure.

M. Tillaux, surgeon to the Hospice of Bicêtre, has lately reported a case of trephining of the spine, in a paper published in the *Bulletin Général de Therapeutique*, 15th March, 1866. In his communication M. Tillaux makes some very judicious observations on the symptoms which indicate or contraindicate operation in cases of fracture of the vertebral column. One is surprised, therefore, at finding that he himself selected a case eminently unsuited for it. The patient died on the night succeeding the operation, and M. Tillaux himself observes:—"Il est facile de voir par l'observation, que la mort a été le résultat d'une myélite ascendante qui existait déjà, et dont la marche a pu être seulement activée par l'extraction des fragments vertébraux.—Il ne serait pas juste de rendre l'opération responsable du résultat." In this last sentiment most surgeons will concur, and it is to be hoped that others who may look favourably on the operation will not select cases for its performance in which there cannot be the faintest hope of its being successful.

ART. II.—*On the Action of the Heart.* By GEORGE PATON, M.D., Toronto.

IF we expose the heart in a living animal, and examine its action, we will find that the auricles contract whilst the ventricles dilate, and the ventricles contract whilst the auricles dilate, and are refilled with blood from the veins. The auricles, in contracting, approximate closely to the ventricle—shoot, as it were, into the auriculo-ventricular foramen, projecting the blood into the cavity of the ventricle, and the ventricle expands, swells out, and becomes distended with blood, assuming a dark red colour. These two movements—the contraction of the auricles and the dilatation of the ventricles—appear simultaneous, being performed at the same instant of time; the ventricle then contracts, diminishing in all its dimensions, propelling the blood along the aorta and pulmonary arteries, synchronous with which the auricles dilate, and receive more blood from the venous trunks.

The auricles, in contracting, approach closely to the orifice of the ventricle, which enables them with greater facility to project the blood into its cavity. This is distinctly seen in the heart of the

turtle, where the auricles in contracting shoot towards the foramen of the ventricle, leaving vacant at their upper extremity a portion of space which they occupied, but which they again immediately cover, as they stretch out and extend their parietes during dilatation. The ventricle, in dilating, enlarges in all its dimensions, its apex descends a little lower in the thorax, dipping towards the spine; its transverse diameter is increased, and anterior parietes rendered protuberant; and in some animals the margin of the base is extended upwards, so as partly to cover or conceal the auricles during their contraction. When the ventricle contracts its apex is again drawn up and slightly tilted forwards, and the body of the ventricle, much diminished in size, appears distinct from the auricles which have receded, and are again dilated, the blood flowing into them from the veins.

When the heart pulsates with vigour, and respiration is well maintained, rendering the blood duly arterialized, the contraction of the ventricle is scarcely finished before the auricles again contract and shoot a wave of blood into the ventricle, as it expands, and quickly attains its maximum of dilatation. It immediately contracts whilst the auricles dilate; and these movements occur in rapid succession, the eye being scarcely able to follow them, and analyze them distinctly; but after careful investigation and numerous experiments, we are satisfied that during this state of the system the dilatation of the ventricle is one distinct or individual act quickly performed, and that no pause or period of repose occurs during its completion. If, however, the action of the heart be slow, and the blood imperfectly arterialized the dilatation of the ventricle attains a pause before the auricles contract; and on their contraction the blood which they project into the cavity of the ventricle distends its walls, or, in other words, completes the dilatation—which phenomenon is distinctly seen in cold-blooded animals when the circulation is languid, the pulsations of the heart not exceeding 20, 25, or 28 per minute.

The circumstance of a pause occurring during the dilatation of the ventricle, in connexion with other phenomena observed in the action of the heart has led physiologists to conclude that it is only during the contraction of the ventricle that an active power is exerted, and not during its dilatation. Accordingly, it is an opinion very generally entertained by physiologists that the diastole of the ventricle is produced by the relaxation of the muscular fibres—by their returning to a state of rest, during which the blood flows from the distended auricles into the cavity of the ventricle

independently of contraction; that a pause then ensues, succeeded by the contraction of the auricles projecting more blood into the ventricle, and bringing it into a state of distention; hence the dilatation of the ventricle is supposed to consist of two stages: during the first stage the blood flows from the auricles into the cavity of the ventricle to supply the vacuum that is formed by the receding walls of the ventricle as it assumes a state of rest; during the second stage, the blood is propelled by the contraction of the auricles into the ventricle already partially filled to complete the dilatation. But during both these stages the ventricle is supposed to exist in a passive or relaxed condition.

This view has been very distinctly expressed by Dr. Hope in his *Analysis of the Movements of the Heart*. He divides, as did Lænnec, a beat of the heart into several parts—that is, from the commencement of one pulsation to the corresponding period in the next:—"1st. The ventricular systole occupies half the time, or thereabouts, of a whole beat. 2nd. The ventricular diastole occupies a fourth, or at most a third. 3rd. The interval of ventricular repose occupies a fourth, or rather less, during the latter half of which the auricular systole takes place." This makes a whole beat consist of four parts. The ventricular systole occupies two-fourths of a beat, the ventricular diastole one-fourth, and the period of repose one-fourth, during the latter half of which the auricle contracts, projecting the blood into the ventricle, thus allowing one-eighth of a beat for the period in which it is performed.*

The data on which these conclusions rest appear to have been limited to the action of the heart when it is slow, and the blood imperfectly arterialized; at least this is the description of the action of the heart of an animal in that condition. This phenomenon is distinctly seen in cold-blooded animals during the period of Winter, or when they are reduced to a state of partial asphyxia, the action of the heart being much depressed, amounting to ten or twelve pulsations per minute. In the frog, to which Dr. Hope's observations refer, the ventricle, on contracting, immediately dilates, its apex being extended a little lower, and its parietes expanding, so that blood flows into the cavity from the auricles, partially distending it. A pause then ensues, during which the ventricle and auricles remain quiescent; the auricles then contract, propelling

* Dr. Hope on Diseases of the Heart, p. 20.

more blood into the ventricle, which causes the anterior parietes to bulge out or protrude slightly in the centre—on which the ventricle immediately contracts, becomes small and pale, propelling the blood along the aorta or *bulbus arteriosus*, as it is termed; and these movements continue in regular succession. But if the animal respire at times the action of the heart is accelerated, amounting to fifteen or twenty pulsations in the minute, the pause becomes shorter, and the movements are performed with greater vigour.

If we were permitted to reason *a priori*, we might conclude that the action of the heart, which is suited to a condition of the system in which all the functions are depressed, would not be adapted to the circulation of an animal whose heart beats at 60, 80, or 90 pulsations per minute—the blood moving with great velocity, and passing through the heart once every three or four minutes, or twenty times in the hour. But science requires data founded on strict observation, and a rigid induction of facts; and many physiologists, in order to determine this question, have observed the action of the heart in living animals whose respiration was fully maintained, and the circulation vigorous, and have failed to perceive the pause or the two stages of dilatation. Majendie, who frequently denuded the heart of living animals, observes that the action of the auricles and ventricles alternate, but makes no mention of a pause. And Professor Cruveilhier, who had an opportunity of investigating this subject in the case of a child whose heart was protruded without the walls of the chest—the auricles and ventricles dilating and contracting with vigour, and respiration being fully established, states:—1st. That the contraction of the auricles was synchronous with the dilatation of the ventricles. 2nd. That no pause or period of repose occurred during the dilatation of the ventricle. 3rd. That the auricles and ventricles dilated with a force, which afforded every indication of being an active vital movement.

And our researches on the action of the heart in living animals lead us to conclude:—1st. That when the action of the heart is slow, and the blood imperfectly arterialized, a pause occurs during the dilatation of the ventricle. 2nd. When the action of the heart is vigorous the pause disappears, and the dilatation of the ventricle takes place simultaneously with the contraction of the auricles. 3rd. That the diastole affords evidence of being an active power as well as the systole.

When the blood is highly arterialized, and the action of the heart vigorous, the dilatation of the ventricle consists of an

individual, or continuous act quickly performed in unison with the contraction of the auricles. If we examine the action of the heart under these circumstances, as the heart of a frog, we will perceive that the contraction of the auricles is synchronous with the dilatation of the ventricle, which annihilates the pause; and that the ventricle as it dilates not only extends its apex a little lower, but rapidly stretches its parietes upwards, so as almost to conceal the auricles, as they contract, and project the blood into its cavity—a movement opposed to that of relaxation, and evidently dependent on some inherent power possessed by the ventricle itself. And if we observe the dilatation of the auricles, as it appears in the heart of the turtle, we will find that it consists in a rapid extension of the parietes in a longitudinal form till they have attained or reached their extreme limit, thus enlarging the capacity of the auricles as the blood flows into them from the veins.

And the increased rapidity with which the ventricle dilates, as the circulation of the blood is accelerated, could not be explained on the principle of relaxation, but decidedly indicates a vital active movement.

If the heart pulsates at the rate of thirty beats per minute, or one beat in two seconds, the contraction of the ventricle, according to Dr. Hope and others, occupies one half of this period or one second; and the first stage of the dilatation, which is described as taking place instantaneously, occurs in one-fourth of the beat, or half a second—considered to be dependent on the relaxation of the fibres of the ventricle, bringing it into a state of rest; the remaining one-fourth of the beat, or the half second, being occupied by the pause or period of repose, and the contraction of the auricles, increasing the amount of blood contained in the cavity of the ventricle, and bringing it to a state of distention. If the heart pulsates at the rate of sixty beats per minute, or one beat per second, the contraction of the ventricle occupies one half of this period, or takes place in half a second, and with twice as great velocity as when the ventricle contracts at thirty beats per minute; and the first stage of the dilatation which occupies one-fourth of a beat is also accomplished in one-half the time, or with double the velocity that it is when the heart beats at thirty pulsations per minute, the remaining one-fourth of the beat being occupied by the period of repose, and the auricles contracting at double the velocity, bringing the ventricle to a state of distention; and if the heart pulsates at the rate of ninety beats per minute, or three beats in two seconds, the ventricle contracts at

the rate of three times the velocity that it does when the heart pulsates at thirty beats per minute; and the ventricle also dilates with three times greater rapidity, showing that the dilatation of the ventricle is regulated by exactly the same influence and under the same law as its contraction, and that its diastole is as much an active power as its systole.

If the dilatation of the ventricle takes place with various degrees of rapidity, according to the force and velocity with which the ventricle contracts, and in order that the circulation may be carried on at a certain rate of speed, then, it must be the result of a vital power as well as the contraction, and not the mere passive relaxation of the muscle. Again, if the dilatation of the ventricle be accomplished in one-third of the time, the heart beating at ninety per minute, that it is when the heart pulsates at thirty per minute; it follows that the blood must pass from the auricle into the ventricle and distend it in this minimum period. But if there be two stages of the dilatation, the auricle contracting during the latter half of the second stage, it leaves at this rate of speed the auricle only one-twelfth of a second to contract and complete the dilatation of the ventricle, a rapidity at variance with all that we have seen of the contraction of the auricle and dilatation of the ventricle during a similar state of the circulation. For when the blood moves with great velocity, the contraction of the auricle commences simultaneously with the dilatation of the ventricle, and the ventricle without any intermission speedily attains its maximum of dilatation.

Whilst the circulation is maintained at this degree of rapidity, the dilatation of the ventricle in order of rhythm and of time precedes its contraction. For the contraction of the auricles, as is well known, takes place before that of the ventricles; and as the dilatation of the ventricles is synchronous with the contraction of the auricles, it is evident that it must be considered to precede and not to follow the systole of the ventricle. When the heart pulsates at the rate of sixty beats a minute, a speed similar to what obtains in many warm-blooded animals in a state of health, the contraction of the ventricle is scarcely finished before the auricles contract, and the moment the jet of blood enters the cavity of the ventricle it expands, and the diastole is quickly completed and succeeded by the systole. And here nothing like a suction power is exerted by the ventricle, the blood being distinctly projected by the auricle as it contracts. Nor does any pause or period of repose intervene from the commencement of the dilatation till its completion.

But when a pause occurs during the diastole of the ventricle, that is to say, when the action of the heart is comparatively slow, the diastole of the ventricle is considered to follow the systole, and not to precede it,^a because the first stage of the dilatation and the pause is completed before the auricles contract, and shoot down to the foramen of the ventricle to complete the second stage of the diastole, on which the ventricle immediately contracts.

Haller, who seems to have viewed the action of the heart when it was slow, and who strongly maintained the doctrine of the relaxation of the ventricles, enquires the reason of the particular order or occurrence of the movements—that is, why the auricles contract, whilst the ventricles dilate, and dilate whilst the ventricles contract; and then the ventricles repose dilated, and the auricles smartly contract? And Dr. Hope, considers the reason to be that the auricles, which have been refilling during the contraction of the ventricles, are brought to a state of distention, by which they contract at the moment of repose, in order that the ventricles by the additional quantity of blood which they receive from the auricles, may then be distended and stimulated to contract. But according to the views which we have advanced, these movements arise as a necessary consequence from, or are intimately connected with the action of the heart when it is slow; and the movements are performed in a different manner when the action is accelerated; the reason of which movements will become more obvious as we proceed.

This subject, however, will be better understood if we describe a few of the experiments we have performed in our researches on the action of the heart.

If we expose the heart of a living fish, whose gills are well protected by the operculum, enabling them to retain a due degree of moisture, sufficient to sustain the function of respiration for a considerable time, after the animal has been removed from the water—and it is surprising to what extent this power is possessed by some fishes—it will be found that when the effect of the operation has subsided, the circulation will be carried on with energy, and the animal will survive for a time, affording ample opportunity to study the movements of the heart.

July 8.—Denuded the heart of a living fish^b, and carefully observed the action of the heart.

^a Dr. Hope on the Heart, p 19.
Commonly termed the *Mud Cat Fish*.

The heart beat at thirty-six pulsations per minute, and the auricle, on contracting, projected the blood into the cavity of the ventricle, which slightly bulged out in the centre, and contracted, the apex being tilted up, and the blood propelled along the bulbus aortæ, the ventricle immediately dilated and a slight pause ensued, during which both auricle and ventricle were quiescent. The auricle then contracted and projected the blood into the ventricle, and the same movements were repeated. After dilatation the ventricle did not move till the auricle contracted, but the auricle which remained dilated, having been refilled with blood from the veins during contraction of the ventricle, contracted shortly after the pause ensued. During contraction the ventricle was drawn up or contracted towards the bulbus aortæ; but during dilatation the fibres were stretched out or elongated towards the abdomen; and the contraction seemed to occupy longer time than the dilatation.

The pulsations increased to forty-two per minute, and then the auricle commenced to contract just as the ventricle reached the pause—that is, simultaneously with the completion of the dilatation of the ventricle.

But on the pulsations amounting to fifty per minute, the contraction of the auricle commenced before the dilatation of the ventricle was completed, or before it came to a pause. The moment the auricle contracted and propelled the blood into the ventricle, the dilatation was completed, and the ventricle contracted; and the reason apparently was, that the ventricle was stimulated or excited by the action of the auricle as it projected the blood into its cavity; and in this case there was no pause or period of repose, the act of dilatation being distinctly continuous.

During these movements of the heart the function of respiration was maintained, as the blood passed along the bulbus aortæ with great rapidity to the gills, which were at times moistened with water sprinkled over them; and we observed that when the animal respired, opening and shutting the operculum with vigour, the action of the heart was quickened.

July 4th.—The heart was exposed in another living fish, and the phenomena beautifully seen, as the animal survived long, and the heart continued to act with great vigour, pulsating at sixty and sixty-four per minute.

When the auricle contracted or projected blood into the ventricle, it expanded and bulged slightly out in the anterior

parietes, extending its apex rapidly outwards, and then contracted towards the bulbus aortae, the apex being distinctly tilted up. The contraction was no sooner terminated, and the dilatation commencing, than the auricle again contracted and sent a wave of blood into the ventricle; and the same movements were repeated, and continued without interruption, the ventricle dilating and contracting in quick and regular succession; the contraction of the auricle was now synchronous with the dilatation of the ventricle, and the pause or period of repose entirely disappeared.

On one occasion, when the animal had struggled much, and turned upon its side, the ventricle became empty and quiescent, and we observed, on the animal being re-adjusted, that the moment the auricle contracted and projected blood into the ventricle, it dilated and then contracted; the same effect we have frequently seen produced in other instances, from which we inferred that the blood projected by the auricle exerts an influence on the dilatation of the ventricle.

In other fishes, on rapidly denuding the heart, we have found the auricle, ventricle, and bulbus aortae acting with great energy, at the rate of eighty pulsations per minute; the auricle contracting towards the orifice of the ventricle, and the ventricle contracting towards the bulbus aortae, on which occasion there was no pause nor anything approaching it, in the dilatation of the ventricle; and no evidence that any blood flowed from the auricle into the ventricle, by a suction power, to fill up a vacuum. But the blood which entered it was distinctly projected from the auricle during its contraction, which was synchronous with the dilatation of the ventricle; hence there was not two stages of the dilatation, but one, and that one rapidly performed.

June 16th.—Denuded the heart of a living turtle,^a and observed its action. Respiration was not affected, the animal continuing to respire at times, as is usual with cold-blood animals.

After the operation the heart beat at sixteen pulsations per minute.

In the Turtle, the auricles, on contracting, shoot down in a lateral direction to the foramen of the ventricle, leaving vacant a space at the extremity of each to the extent of one-fourth of an inch, which they again cover as they fall back, and stretch out their parietes during dilatation. After contraction the auricles immediately

^a Mud turtle.

dilate, and during dilatation the blood flows into them from the venous trunks; then they remain distended and quiescent for an instant, till the next contraction.

When the ventricle contracts its base is raised up towards the bulbus aortæ, along which it propels the blood; and this vessel becomes more curved, hard, and tense at the moment; after contraction the ventricle immediately dilates, and a pause ensues, during which the auricles and ventricle are quiescent. Then the auricles contract and propel more blood into the ventricle, completing the dilatation, or as it has been termed, producing distention of the ventricle, on which it instantly contracts, and again dilates, attaining a pause as before; and these movements continue.

When the heart beat at 22 pulsations per minute, its action was more vigorous than when it beat at 16 per minute, and the pause of shorter duration; in other words, the auricles contracted sooner, propelling the blood into the ventricle—completing the second stage of dilatation.

The action of the heart having increased to 28 pulsations per minute the auricles contracted as the dilatation of the ventricle attained the pause.

But on the pulsations increasing to 32 per minute the auricles contracted, projecting the blood into the cavity of the ventricle as it dilated or proceeded to a pause. The dilatation was completed, and the ventricle contracted, but no pause occurred—the dilatation consisting of one individual act.

When the action of the heart attained to 36 or 38 pulsations per minute, the contraction of the auricle was synchronous with the dilatation of the ventricle—that is to say, the auricles commenced to contract, and shoot down to the foramen of the ventricle as the ventricle began to dilate. It almost appeared as if the blood which the auricles projected into the cavity caused its dilatation—the ventricle expanding and dilating as the wave of blood entered it. The auricles dilated and received more blood from the veins, whilst the ventricle contracted, and the instant the auricles were refilled with blood they contracted, whilst the ventricle dilated; these movements being performed in rapid and uniform succession. But the contraction of the ventricle seemed to occupy longer time than its dilatation. Then there was no pause, the dilatation of the ventricle consisting of one act—synchronous with the contraction of the auricles.

Neither the auricles nor ventricle appeared to empty themselves

during the contraction, expelling only a portion of the blood which they contained.

July 21.—The heart of another living turtle was denuded, and the phenomena observed. Respiration continued; and after the effects of the operation had subsided, we examined the action of the heart, and found it 25 pulsations per minute.

The dilatation took place immediately after the contraction of the ventricle, and proceeded to a pause. The auricles then contracted, projecting more blood into the ventricles, completing the dilatation—on which the ventricle contracted.

But when the pulsations attained to 36 per minute, the auricles contracted simultaneously with the dilatation of the ventricle; that is, after the contraction was finished, and the dilatation of the ventricle commencing, the auricles contracted, projecting a wave of blood into the ventricle, and the dilatation was completed. They immediately receded, extending or stretching their parietes outwards, and dilated, receiving more blood from the veins, whilst the ventricle contracted, propelling the blood along the *bulbus aortæ*. And these movements were continued, the contraction of the auricles coinciding with the dilatation of the ventricle—the pause disappearing—and the dilatation consisting of one act quickly performed; 36 pulsations per minute were the highest attained.

During all these experiments respiration was continued, the animals respiring at times, as is done by cold-blooded animals, and then the action of the heart was accelerated.

When the animal became agitated and struggled much, we have seen the ventricle contract and dilate for several successive times, whilst the auricles remained motionless and distended with blood; but on the auricles contracting, the regular and uniform action of the heart was restored.

As regards the dilatation of the auricles, we cannot suppose that it was produced by the mere influx of blood into the cavity, for it was during the extension of the parietes that the blood flowed from the veins, and the veins have not the power, by the blood which they pour into the auricles, to produce such a movement. It was not like distention, but an actual expanding or stretching out of the parietes of the auricles, as if by an innate power which they possessed.

July 15.—Denuded the heart of a frog which had been reduced to a state of partial asphyxia—carefully guarding against injuring the muscles of the throat, on which the function of respiration depends.

After the effects of the operation had subsided, we examined the action of the heart, and found it 30 pulsations per minute.

The ventricle having contracted, and propelled the blood along the bulbus aortæ, immediately dilated, becoming partially filled with blood, the apex being stretched out or extended lower, a pause ensued, during which the auricles contracted, projecting more blood into the ventricle, which expanded its parietes, and extended its base upwards—thus completing the second stage of dilatation and contraction, and contraction immediately ensued. Sometimes the first stage of dilatation was so closely succeeded by the contraction of the auricles, and increase of blood into the ventricle, that it seemed almost one continuous act. The first dilatation was very short, and quickly performed, and the principal act of dilatation was when the auricles contracted, and projected the blood into the ventricle. It was not distention, but an actual stretching out and expanding of the parietes of the ventricle, so as greatly to enlarge its capacity, and it instantly contracted.

The action of the heart increased to 36 pulsations per minute, and the dilatation of the ventricle was now continuous—no pause—the auricles contracting, and projecting the blood into the ventricle as it dilated and extended its parietes—assuming a dark red colour. It immediately contracted, and these two movements were continued—the ventricle dilating as it received the blood from the auricles, and contracting as it propelled the blood along the bulbus aortæ. In dilatation, the apex of the ventricle descended a little lower, dipping towards the spine, the anterior parietes became protuberant, and the margin of the base was rapidly extended, partially concealing the contracting auricles from the view. In contraction the ventricle became small and pale, being apparently emptied of its blood, and the apex was slightly tilted up. Whilst the heart acted with this degree of vigour, the animal respiring freely and remaining at rest, the pause or period of repose disappeared in the dilatation of the ventricle—the dilatation consisting of one distinct act, because the auricles contracted sooner in the order of time and of rhythm, and as the ventricle dilated.

The animal became restless and agitated, and the ventricle lay contracted and motionless; but on the animal becoming quiescent, the auricles contracted or shot down to the orifice of the ventricle and that moment the ventricle expanded or opened up its parietes, the blood being projected into its cavity. It contracted as the auricles receded or dilated, and the movements were continued in

regular succession, and were always more vigorous when respiration was renewed and continued for a time.

The animal having remained at rest for a considerable time, respiration being maintained with vigour, the action of the heart increased to 44 pulsations per minute.

The instant the auricles dilated, and were refilled with blood from the veins, they contracted or shot down to the foramen of the ventricle, and the ventricle rapidly dilated, opened up, and extended its walls as the blood entered it. It immediately contracted, propelling the blood along the bulbus aortæ; and the same movements were repeated and continued. The dilatation of the ventricle was now synchronous with the contraction of the auricles, and it seemed as if the blood projected by the auricles excited the ventricle to dilate; the instant it touched the ventricle, the ventricle opened up, and expanded its parietes, and was filled with blood. There were thus two distinct forces in operation—the auricles contracting and projecting the blood into the ventricle, and the ventricle expanding its parietes and enlarging its capacity to receive it.

September 12th.—Denuded the heart of a living frog, leaving the muscles of the throat uninjured. Respiration was continued and maintained with vigour.

The heart beat at 60 pulsations per minute; and as the auricles shot down to the foramen of the ventricle, or contracted, projecting the blood into its cavity, the ventricle dilated, its fibres being distinctly stretched out or elongated as usual, it immediately contracted, diminishing in all its dimensions, whilst the auricles dilated. And these movements went on in regular succession, the ventricle dilating as the blood was projected into it during contraction of the auricles, and immediately contracting and propelling the blood along the bulbus aortæ. But after contraction of the ventricle it did not dilate before the auricles contracted; in other words, the contraction of the ventricle was just terminated when the auricles shot down to the foramen of the ventricle, or contracted; and that moment the ventricle dilated, expanding its parietes and extending its apex as the blood flowed into it, and then contracted with great energy, the blood being expelled. The action of the ventricle consisted of dilatation and contraction, maintained without interruption, the dilatation of the ventricle being synchronous with the contraction of the auricles, and its contraction coinciding with their dilatation, the dilatation of the ventricle being one distinct act, without any intervening pause.

October 5th.—Denuded the heart of a living frog, and found the pulsations 80 per minute. Respiration was maintained with energy.

The ventricle dilated whilst the auricles contracted and projected the blood into its cavity; and it contracted whilst the auricles dilated, diminishing in all its dimensions, and propelling the blood along the *bulbus aortæ*. There was no pause or interval of repose between the dilatation of the ventricle and contraction of the auricles. This fact was clearly and distinctly ascertained, on some occasions the ventricle having contracted, and being small and pale, the auricles shot down to the foramen of the ventricle, or contracted; and that instant the ventricle expands or dilates, assuming a florid red colour. But no dilatation of the ventricle took place, and no blood entered its cavity except during contraction of the auricles—whilst they contracted the ventricle dilated, and *vice versa*—the movements being repeated and continued in quick and alternate succession.

On carefully observing the action of the ventricle, it appeared when the ventricle dilated, as if its fibres were stretched out or extended to a certain extent; and then drawn up and retracted during contraction. The dilatation appeared the first or principle act, and the contraction succeeded it.

There was a unison in all these movements. When the action of the heart was increased or diminished a little, the contraction of the auricles which took place immediately on being dilated, was so regulated that it occurred just as the contraction of the ventricle was finished, rendering the dilatation of the ventricle synchronous with the contraction of the auricles; and they were dilated and refilled with blood during the contraction of the ventricle.

The movements of the ventricle then consisted of dilatation and contraction, the former being one distinct and individual act, without an intervening pause. The ventricle dilating and then contracting as a consequence of its dilatation.

During contraction, the ventricle appeared to expel all the blood which it contained, as it became small and pale. But during contraction the auricles expelled only a portion of their contents, as they never appeared to be completely empty.

We may also state, when the heart was quickly denuded in these animals, its action was often observed to be maintained with great vigour. In the fish and the frog the *bulbus aortæ* contracted immediately after and in unison with the contraction of the ventricle. And in the turtle immediately after contraction of the ventricle, as

the wave of blood was passing along the aorta or bulbus aortæ, that vessel became more curved, hard, and tense; all of which movements were evidently designed to impart an impulse to the blood in its onward course, as it proceeded to the organs of respiration, or to be distributed throughout the body.

These experiments will enable us to judge respecting the manner in which the dilatation of the ventricle is accomplished, and the action of the heart increased. It appears that when the action of the heart is weak and languid the ventricle dilates without much apparent vigour, and attains a pause, before the contraction of the auricles commences, and completes the dilatation. This is distinctly seen in cold-blooded animals, when the heart pulsates at 20 to 25 per minute. 2nd.—When the heart acts with greater energy, the contraction of the auricles coincides with the pause, or shortly precedes it. And third, when the heart beats with great rapidity, the auricles contract simultaneously with the dilatation of the ventricle; so that no pause, nor anything approaching it, is manifested, but the dilatation is completed by a quick and continuous movement, and accomplished in a much shorter time, than when a pause intervenes, before the auricles contract, and project their blood into the cavity of the ventricle.

What is the cause of this increased action of the heart, the animal remaining in a calm and quiescent state, without manifesting symptoms of agitation or uneasiness? It seems to be intimately connected with the condition of the blood. While the blood continues dark and imperfectly arterialized, the action of the heart is weak and depressed, and a pause occurs during the dilatation of the ventricle, before the auricles contract and project their blood into its cavity. But as soon as respiration commences or is increased, the action of the heart improves, and a pause can scarcely be discovered in the dilatation of the ventricle, the auricles contracting before the commencement of the pause, or coinciding with it. And when respiration is fully established and maintained, the heart pulsates with great vigour and no pause occurs, the contraction of the auricles being synchronous with the dilatation of the ventricle.

On the other hand, if a high rate of speed abates, and the action of the heart becomes less vigorous, the auricles are later in contracting and in projecting their blood into the dilating ventricle, till at length a pause intervenes before the auricles contract.

We perceive then how the action of the heart is adapted to the

different rates of speed in the circulation of the blood, according to the condition of the animal, and the circumstances in which it is placed, and how the circulation is increased in unison with the demands of the system. The auricles contract sooner in the order of time, overtake as it were the ventricle in dilating, or actually commence with it, whilst the ventricle at the same instant manifests vital energy and activity in the increased rapidity with which it dilates, and receives the blood into its cavity.

Let us now enquire how the doctrines we have enunciated apply to the action of the heart in warm-blooded animals, and the phenomena there observed; premising, that the movements of the heart are acknowledged to depend on the same principle in all vertebrated animals; physiologists being agreed that the heart of the frog pulsates in the same manner, and observes the same laws, as the heart of the rabbit, ass, horse, and other warm blooded-animals. Indeed, during the heat of Summer, when the cold-blooded animals assume the physiological condition of the warm-blooded, the action of the heart appears to be identical, not only as regards the principle on which the movements depend, but the quickness and rapidity with which they are executed and maintained—the pulsations in many of them amounting to 70 or 80 per minute.

Dr. Hope informs us, in his experiments on the ass, whilst investigating the sounds of the heart, he found that, when its action fell to the natural standard, from 40 to 50 beats a minute, the pause occurred during the dilatation of the ventricle. “Three motions,” he states, “could be distinctly recognized, namely, the auricular systole, the ventricular systole, and the ventricular diastole.”^a Now, we know that during the auricular systole the blood is projected from the auricles into the ventricles; and during the ventricular systole the blood is propelled from the ventricles along the aorta and pulmonary artery; and the ventricular diastole is the dilatation of the ventricle terminating in the pause.

He also states, in another part of his work, that “when the animal which was the subject of experiment, regained the slightest degree of sensibility, the action of the heart was so violent, convulsive, and rapid, as to present the appearance of alternate action described by Majendie;” or, in other words, that a pause could not be satisfactorily recognized during the dilatation of the ventricles. And “that in small animals also, as rabbits, whose pulse beats 150 to 200 a

^a Dr. Hope on Diseases of the Heart, p. 16, 17.

minute, the same appearance is generally presented, even though they have been completely killed, for the interval of repose is too brief to be appreciated by the eye." "Nay, in asses poisoned by woorara, much the same appearance is presented, whenever the pulse is accelerated 20 or 30 beats above its natural standard of 40 or 50, the contraction of the auricle, then becoming more active and extensive, and encroaching so much on the interval of repose as to render it indistinct to an unpractised eye. I am therefore inclined to think that during palpitation or unnaturally accelerated action, the period of repose actually is encroached on."

From these statements it appears that Dr. Hope was unable to perceive the pause or period of repose in the action of the heart in warm-blooded animals, when it was rapid and amounted to 50 beats and upwards a minute; and the reason is, that according to a general law in the action of the heart, when the circulation is maintained with vigour the pause does not exist, but disappears. It can be satisfactorily shown that in this condition of the system not only is the pause or period of repose encroached on, but the contraction of the auricles, as we have seen, anticipates that period, and takes place simultaneously with the ventricular diastole. A fact which has been distinctly recognized by many physiologists in their experiments on the action of the heart in warm-blooded animals.

To illustrate this subject let us consider the action of the heart in some warm-blooded animals when it is slow and the pause visible, and then where it is accelerated—remembering that the movements of the auricles are simultaneous, as well as the movements of the ventricles; and that the description of one comprises that of the other.

The ventricles contract and propel the blood along the aorta and pulmonary artery, and the ventricular diastole immediately takes place, during which there is an influx of blood from the auricles to the ventricle partially filling it; a pause ensues, and the auricles contract, propelling more blood into their respective ventricle, which expands its walls as the blood enters it, becoming fully dilated; and instantly contracts, propelling the blood along the aorta and pulmonary artery; the ventricular diastole again takes place, and the movements are continued. But on the action of the heart becoming accelerated, instead of a portion of blood passing from the auricles to the ventricle, partially dilating it, it is all projected by the contraction of the auricle, completing the diastole at

once, instead of two successive stages; and the same movements are repeated and continued without intermission. What is the consequence? The auricles contract immediately after the contraction of the ventricles is finished, and not as formerly, after the completion of the first stage of the dilatation, which circumstance annihilates the pause; and as the dilatation of the ventricle is now completed at once, and synchronous with the contraction of the auricles, it may be considered in order of rhythm and of time to precede the next ventricular systole, so that the movements of the ventricle are converted into the diastole and the systole; during the former, the blood is projected by the auricle into the cavity of the ventricle; and during the latter, the blood is propelled by the ventricles along the aorta and pulmonary artery.

Dr. Hope, and the physiologists who adopt his views, consider the dilatation of the ventricle during the first stage, as it proceeds to a pause, to occur after the systole; and the second stage of the dilatation, which is connected with the contraction of the auricles, to precede the systole. The first stage of the dilatation he views in relation to the preceding systole, and the second stage in relation to the succeeding systole. But if during the vigorous action of the heart in warm-blooded animals the contraction of the auricles coincides with the diastole of the ventricle, and this be completed at once, and not at two successive stages—we are entitled, on the same principle as that adopted by Dr. Hope to consider the diastole to precede the systole—that is, to view it in relation with the succeeding systole; and this is, undoubtedly, what took place in the case of the child, where the heart was protruded without the walls of the chest, and the action was so carefully observed by Cruveilhier. The contraction of the ventricles is stated to have been precisely synchronous with the dilatation of the auricles; and the dilatation of the ventricles to have been performed at the same time with the contraction of the auricles; no period of repose intervening between the two sets of actions;^a and Majendie states:—“If the heart of a living animal is denuded, we easily see that the auricles and ventricles contract and dilate alternately. These movements are so arranged that the contraction of the auricles takes place simultaneously with the dilatation of the ventricles, and *vice versâ*; that the contraction of the ventricles coincides with the dilatation of the auricles.” And our views are to the same effect.

^a Carpenter's Physiology.

We have denuded the heart of living warm-blooded animals, and carefully observed its action. We have found the pulsation in some cases 90, in others 120, &c., per minute; but we could only recognize two movements in the ventricle, namely, its dilatation and contraction. The dilatation took place simultaneously with the contraction of the auricles, and the systole coincided with their dilatation. There was nothing like an intervening pause, or anything approaching it, in the dilatation of the ventricle.

These facts, established by careful observation, undoubtedly prove that when the dilatation of the ventricle is synchronous with the contraction of the auricles, being completed in one act, it must be considered to precede the ventricular systole, and not to succeed it.

This view may appear at variance with the fact that the second sound of the heart which immediately follows the first is usually considered as coinciding with the ventricular diastole, when the auricles are considered to contract a little later, and to be quickly succeeded by the contraction of the ventricles. In explanation of which we must remember that when the ventricles contract and propel the wave of blood along the aorta and pulmonary artery, the contractile or elastic power which these vessels exert in assisting the circulation forces a portion of the blood against the semilunar valves and produces the second sound the moment the ventricular systole ceases or the *vis a tergo* is withheld. In the fish and frog the bulbus aortæ contracts immediately after the contraction of the ventricle, and assists in propelling the blood forwards. And in the higher order of animals the contractile or elastic power possessed by the aorta or pulmonary artery produces the same effect; so that we perceive how the second sound quickly succeeds the first. And if we adopt the more recent view,* that the first sound of the heart is produced chiefly when the blood from the ventricle enters the aorta and pulmonary artery, we are enabled to explain not only how the second sound quickly follows the first, but how the greater interval elapses between the occurrence of the second and that of the first sound—the period of silence in the healthy action of the heart.

With the second sound of the heart commences the dilatation of the ventricles, and simultaneously with this the auricles contract; and as the blood is projected into the ventricle it expands, swells out, and becomes freely dilated. Here the act is more prolonged

* Carpenter's Physiology, page 417.

than during the first stage of dilatation, because the whole of the blood that enters the ventricles is projected by the auricles. And the interval is greater between the commencement of the contraction of the auricles and the contraction of the ventricles; in other words, the full dilatation of the ventricles is completed—in unison with the contraction of the auricles—and then the ventricle contracts.

But how do these views harmonize with the description that has been recently given of the direct mitral murmur, that is the murmur produced by contraction of the mitral valve, and manifested when the blood flows from the auricle into the ventricle? It is well known that this murmur has generally been associated with the second sound of the heart;* because it was believed that then the ventricle rapidly dilated and admitted an influx of blood from the distended auricles. But it was considered that though the contraction of the valve might exist to a considerable extent, the murmur was of rare occurrence, owing to the weakness of the current of blood flowing from the auricle into the ventricle, preceding the pause or rest of the heart. And it was also maintained, that when the auricles contracted the force which they exerted was small, and the ventricle being already full the resistance required to be overcome before an extra quantity of blood could enter the ventricle necessarily retarded the force and velocity of the current, and consequently weakened or suppressed the murmur.

But Dr. Gairdner, as the result of his pathological researches, is decidedly of opinion that the direct mitral murmur is of frequent occurrence, and precedes the first sound of the heart, rather than succeeds the second sound, and that its real place in the rhythm of the heart is misrepresented when it has been viewed as a diastolic murmur, and not as produced whilst the auricles contract and transmit the blood into the ventricles. Hence he has termed it an auricular-systolic murmur. But as Dr. Gairdner at the same time admits that the diastole and the pause precede the contraction of the auricles, and as, according to that physiological view of the action of the heart, one-eighth of a beat is allowed for the auricles to contract and pour the blood into the ventricles—which at the rate of 60 pulsations per minute is one-eighth of a second, and at 90 pulsations per minute is one-twelfth of a second—it is difficult to perceive how this corresponds with the character of the murmur, as prolonged, and extending through the pause, and at times

* Dr. Hope on Diseases of the Heart, p. 78 and 388.

appearing to arise from the second sound. Dr. Gairdner describes the murmur in Case I. "as a pure auricular-systolic murmur, extending, however, through the pause, and, therefore, at times appearing almost to spring from the second sound, increasing afterwards in intensity, so as to abut sharply and with emphasis upon the first sound." And in the second case he says, "the other murmur is clearly endocardial—that is, the auricular-systolic murmur, being long and rough, extending quite through the pause, and almost beginning at the second sound."^a

We quite agree with Dr. Gairdner in considering the direct mitral murmur an auricular systolic murmur, but we submit that the description which he gives of it, and which is no doubt perfectly correct, goes far to support the view, that the diastole of the ventricle is synchronous with the contraction of the auricles.

In reference to the argument that when the diastole of the ventricles is synchronous with the contraction of the auricles, it must be considered to precede the systole, since it is admitted that the auricles contract before the ventricles, we may further state: it appears to be the normal action of the heart, that the auricles dilate whilst they receive the blood from the veins, and then contract in expelling it into the ventricles; and if the dilatation of the ventricles be simultaneous with the contraction of the auricles, it follows that this act is not relaxation of the muscular fibres, but must be viewed as a distinct and definite power possessed by the ventricle, by which it opens up and expands its parietes, as the blood flows into its cavity; and then it contracts and projects the blood along the arteries, so that we have an active power exerted both in the dilatation of the ventricle and in its contraction, which alone, in our opinion, is consonant to the high rate of speed maintained in the movements of the heart, the pulsations amounting to 80, 90, or 100 per minute, and even 200 per minute, as in the *menoculus pulex*; and the character of the movement equally indicates an active power, the dilatation being distinctly continuous, of a unique character throughout, from its commencement to its completion. And the same fact is also clearly manifested by the force and vigour with which the ventricle dilates in the higher order of animals. Hence we conclude that the diastole is produced by an actively dilating power inherent in the ventricle.^b

^a Medical Times and Gazette, Oct. 29th, 1864.

^b Dr. Williams remarks, "That the ventricles have an actively dilating power, was held by Bichat, Pechlin, Carson, and others; and even by Lænnec; and although

Professor Cruveilhier states that the diastole of the heart of the child was performed with the power and energy of an active movement, being sufficient to force open the hand that was closed upon it.

From these researches and statements, we think, we are warranted to conclude that when the action of the heart is vigorous, and respiration fully maintained:—1st. The dilatation of the ventricles is synchronous with the contraction of the auricles. 2nd. That no pause or interval of repose occurs during the dilatation of the ventricle. 3rd. The diastole of the ventricles precedes the systole. 4th. The diastole is performed with a power and vigour that affords every indication of a vital active movement.

With regard to the manner in which dilatation of the ventricle is accomplished when no pause occurs—the ventricle must either be possessed of a power within itself by which it attains its maximum of dilatation in unison with the contraction of the auricles, but independent of its influence. For if it precede the contraction of the ventricle, as it undoubtedly does, it cannot be considered the relaxation of the muscle, and its returning to a state of rest. Or the contraction of the auricle by the blood which it projects into the cavity of the ventricle must also stimulate the parietes to extend their fibres and complete the dilatation. But whatever view we entertain, it is evident, that in either case, an active vital power is exerted by the ventricle during the act of dilatation.

During this state of the circulation, the contraction of the auricles takes place, just as the contraction of the ventricle is concluded; and the moment the blood from the auricles touches the ventricle, it expands, swells out, and quickly attains its maximum of dilatation, which would lead us to believe that the blood propelled by the auricle exerts an influence on the rapidity of the diastole of the ventricle. And we have frequently seen, both in the frog and the fish, when, from the agitations and struggles of the animal, the ventricle had become empty and quiescent, that the instant the auricle contracted and projected blood into the ventricle its parietes expanded—dilatation was produced. We have also observed when the auricles have contracted during what is termed the second stage

opposed to what we at present know of animal dynamics, it would be rash to absolutely deny the possibility of its existence . . . whatever be the cause the diastole in large animals is sufficient to force open the hand of a person grasping the ventricles, and it is therefore not surprising that this should have been ascribed to an actively dilating power.”—Williams on the Heart, p. 298.

of dilatation, that the blood as it entered the ventricle produced not merely distention but actual dilatation of its walls, the margin of its base being rapidly extended upwards. And in this case the heart beat at twenty pulsations per minute, but respiration was recommencing, the animal respiring slightly at intervals. And the force and rapidity with which the auricles contract, even admitting the great distensibility of the walls of the ventricle, could not overcome the resisting power and contractile energy, so as to produce distension of the ventricle, already partially filled, and the fibres everywhere in contact with the fluid contained, if the parietes did not dilate to receive the blood projected by the auricles. A smaller force could not overcome a greater.

In the fish the auricle is small and weak in comparison of the ventricle. In the frog also there is much difference between the strength of the auricles and that of the ventricle; and in warm-blooded animals the auricles are weak and unsupported by valves behind, and the current of blood which they project seems inadequate to overcome the contractile power of the ventricles if it did not act as a stimulus to their dilatation. The fact is, that in all these cases there is an adaptation in the ventricle to receive the blood which enters it—an opening up—an expanding or dilating power manifested by the parietes the moment the wave of blood from the auricle touches the ventricle; without which power the heart could never beat with the velocity and regularity it does.

When the dilatation of the ventricle is completed without an intervening pause, there are two forces in operation which can be distinctly seen in some animals. 1st. We have the auricles contracting and projecting the blood with great rapidity into the cavity of the ventricle. 2nd. We have the ventricle quickly extending its parietes, and stretching them upwards to complete the dilatation. It appears like a beautiful piece of mechanism—the auricle contracting and pouring the blood into the ventricle, and the ventricle opening up and extending its walls to receive it, and then contracting and propelling it along the aorta, whilst the auricles dilate and receive more blood from the veins.

When the circulation attains a degree of celerity that the ventricles dilate whilst the auricles contract, or as it is termed, that the dilatation of the ventricles is synchronous with the contraction of the auricles, it is evident the ventricle dilates and then contracts, or in other words, the contraction of the ventricle must be considered the result of the dilatation. If we keep this fact distinctly

in view, and remember that we can discern the fibres of the ventricle stretched out to a certain extent during the dilatation, and drawn up and retracted during contraction, it will be seen that the dilatation of the ventricle is as much a vital act as its contraction.

What is the cause of the dilatation of the auricles? It cannot be produced by the force with which the blood is propelled by the veins, for the venous trunks contract but slightly, and the energy with which the diastole is performed excludes the idea of relaxation, affording every indication of a vital movement. But it appears to be much influenced by the quality of the blood that passes through the heart; for when more duly arterialized blood begins to circulate, the auricles dilate as well as contract with greater vigour. What is the cause of the contraction of the auricles? Is it that, being fully dilated, they are excited to contract? Then, why do they remain for a time filled with blood when the action of the heart is slow? There must be an intimate relation between the quality of the blood and the contraction of the auricles; for when the blood is imperfectly arterialized, they remain quiescent for a little after dilatation has taken place. But when the blood is more duly arterialized, they contract immediately on being dilated; and all this is produced whilst the animal remains at rest, only that the respiration has commenced and been carried on for a time.

We are well aware that the muscular fibres of the heart are considered to be so arranged as to produce contraction of the ventricle, but not to account for its dilatation. But great difficulty was experienced by the late Dr. Duncan, and also the late Dr. John Reid, in unravelling the muscular fibres of the heart in warm-blooded animals, so as to determine their specific action; and we have not as yet been able, from the arrangement of the fibres, satisfactorily to account for the tilting up of the apex of the heart during contraction of the ventricle. And though apparently opposed to the doctrine of muscular contractility, we are decidedly of opinion, from the evidence adduced, that the diastole of the heart depends on an actively dilating power—which power is clearly manifested during increased respiration, vigorous exercise, mental excitement, or whenever the action of the heart is accelerated.*

* We may observe, that continued and increased action does not diminish the power of muscular contractility in the involuntary, as in the voluntary muscles of the body. "In the case of moderate exercise—of mental excitement—and in the course of different febrile and inflammatory diseases—we have examples of greatly increased

What is the cause of the continued action of the heart? This is a question that has much engaged the attention of physiologists. The heart it is well known will pulsate for hours in some cold-blooded animals after life is extinct and the circulation has ceased. The pulsations are continued at intervals after its removal from the body, when no stimulus is apparently present to excite its movements. We have seen in the fish an irregular action of the heart maintained for several hours after life was extinct, the auricle contracting frequently and the ventricle only at times. And these movements continued till the parietes began to dry and shrivel up, and then, after remaining quiescent for a time, a slight movement commenced in the central portion of the auricle, and extended to the ventricle, producing a slight contraction in that part of the ventricle in connexion with the auricle. After a short interval, when all was apparently quiescent, a slight movement was observed in the apex of the ventricle which extended an impulse to the base, and a similar movement, or partial contraction, occurred for several successive times, at the interval of about 30 seconds, the auricle remaining perfectly quiescent. If it be maintained that the atmospheric air acts as a stimulus to the heart when it is empty and deprived of blood, how does it happen that its pulsations are equally distinct and continued under the exhausted receiver of an air-pump? These facts would seem to imply that the heart possesses an inherent power of movement, and that its proper stimulus is the blood—the action of the heart being decidedly affected by the condition and quality of the blood that circulates through the system. Dark and unarterialized blood retards its movements, whilst duly arterialized blood increases its action; under its influence both the auricles and ventricles dilate and contract with great energy and vigour.

action of the heart, without any decidedly debilitating effect on the heart's action necessarily following. The doctrine of increased action, necessary in all cases, lowering or exhausting the irritability of muscles, particularly as applied to the involuntary motions, is therefore by no means an established principle."—Dr. Alison's Physiology, page 39.

ART. III.—*Temperature in Acute Disease.* Being a Thesis read, for the Degree of Doctor in Medicine, before the University of Dublin, June 26th, 1866. By THOMAS ARMETIRDING COMPTON, B.A. of Corpus Christi College, Cambridge; B.A. *ad eundem*, Dubl., and M.B., Dubl.; L.R.C.P., Lond.; M.R.C.S., Eng.

PART I.—ON THE PRACTICAL VALUE OF ACCURATE DAILY OBSERVATIONS OF THE TEMPERATURE OF THE BODY IN ACUTE DISEASE.

I SHALL not attempt to discuss the several theories of the production of animal heat, normal or abnormal, but will merely remark, that I have not, myself, found the actual excess of temperature, in typhus fever, to be proportional, as some assert, to the amount of urea excreted daily, and consequently am inclined to think that abnormal temperatures at any rate are not altogether due to tissue metamorphosis. My observations, however, on this matter, have not been sufficiently numerous to be of much value.

My object in taking up the study of temperatures has been mainly with a view to test their practical value for general use in ordinary practice, and consequently my observations have (with some few exceptions) been taken only once daily, but always as near to the same hour as possible. It would, no doubt, have been more advantageous had several observations been taken during the day at stated hours; but if the value of thermometric registrations generally depends only on a frequent observation of the temperature during the day, a thermometer would necessarily be of little use in private practice. I have, therefore, preferred to register one observation only daily, viz., about 2 p.m., at the same time noting down the state of the pulse, skin, respirations, and general symptoms.

The observations have been, without exception, made in the axilla, the superiority of this situation to that of any other being generally accepted, on account of the nearer approximation of its temperature to that of the blood in the vessels.

The general conclusions which I have come to, up to the present time, have been arrived at after a careful study of some 125 cases taken by myself during the last two years, at St. Bartholomew's Hospital, and also of some seventy-five other cases taken in the same hospital, during the same period, by Dr. Warter, a careful and

accurate observer, to whom I am much indebted for the opportunity of using such valuable material.

I should here mention that during the past six months, the temperature of every case of acute disease, coming into the medical wards of the hospital, has been taken either by Dr. Warter or myself.

The total number of cases, then, in which the temperature and general symptoms have been watched and recorded daily throughout their course, amounts to 200, of which sixty are typhus, thirty typhoid, twenty pneumonia, fifteen scarlet fever, and the remaining seventy-five comprise cases of febricula, acute rheumatism, erysipelas, chorea, acute tuberculosis, &c. The total number of observations in these cases, and in others in which only one temperature has been recorded by Dr. Warter or myself, probably exceeds 5,000. As I shall have to speak of normal and abnormal temperatures, it will be necessary for me to state what I consider to be approximately the average normal temperature of an axilla in a healthy adult. A temperature of 98·4 Fah. is the point generally settled upon by the majority of authorities on the subject; but this, I believe, to be too high, as although I have not at present taken a sufficiently large number to decide the question to my own satisfaction (which I hope, however, shortly to be able to do), yet I can state that I have very rarely found such a temperature present in a healthy adult under normal conditions. I have every reason to think such a temperature to be nearly up to the maximum, consistent with health, and to be only met with occasionally, just as one comes across, now and then, a healthy adult with a temperature below 96° Fah. I consider the healthy range to be somewhere between 95°·5 and 98°·5 Fah., the most common temperature met with, being probably 97°·4 Fah., *i.e.*, one degree less than the temperature hitherto most generally received as the normal one.

The following then are the propositions I wish to establish:—

1st. That a continued daily temperature of 99° Fah., and upwards, indicates an unhealthy condition, and occurs in every case of acute disease. As I have never met with one case in which such a temperature was present, under normal conditions, in a healthy adult, and as every case of the 200 taken exhibits this state of temperature, the proposition may be considered to be proved.

2nd. That any one observation of a very high temperature (such as 105° Fah.), in any case in which the general symptoms do not appear of any particular severity, should lead to a very attentive

re-examination, and suggest a very careful watching, especially if occurring in a non-diagnosed case; such a temperature being present only in severe forms of any disease.

In support of this I will mention the following case:—

W. B., aged twenty-nine, a well-nourished man, of rather sallow complexion, somewhat excited manner, having an occasional spasmodic way of speaking, complained, on admission, of sore throat, and on examination, both tonsils and the back of the pharynx, were seen to be considerably inflamed. He gave an imperfect history of having been ill about a week. This, at first sight, seemed to be merely a somewhat severe case of tonsillitis, and no further very minute examination, by the physician in charge, appeared to be immediately necessary. The case was therefore left for the history, &c., to be made out by the clinical clerk, before the visit next day, as is usual in such cases. Finding, however, the man's temperature to be $105^{\circ}\cdot4$ Fah., I was interested in the case; and I made out, on further examination, the existence of partial trismus, and that the tongue, when protruded, deviated slightly towards the right side. Having mentioned this state of things to the physician, he returned again on purpose to see the case, when he found the symptoms still more severe, and considered the prognosis very unfavourable.

This man, moreover, died on the next day, some twelve hours after admission, the case being one of septicemia.

Another equally important case of cerebro-spinal meningitis, in a girl of eighteen, was thought by many who saw it on admission, to be one of hysteria; but here a temperature of $103^{\circ}\cdot5$ Fah. was quite sufficient to negative such an idea.

This case also terminated fatally.

3rd. That the thermometer is of great use, as a means of diagnosis in those cases, which frequently present themselves, of general *malaise*, often accompanied by a history of rigors, loss of sleep, &c.; such symptoms being due either to the commencement of one of the specific fevers, or merely to some gastric or uterine disturbance of a temporary character.

In these cases it is often at first impossible to decide, judging only from the pulse and general symptoms; but if the state of the patient be due to the more serious cause, we shall invariably find an abnormal temperature; and on the other hand if due to any other, a normal temperature, will be often met with, frequently accompanied by a very rapid pulse. I have never met with one case in which, the temperature being normal, specific fever afterwards developed itself,

and although I have nearly twenty examples of this proposition, I will only bring forward one.

E. J., aged twenty-four, a nurse in a ward in which there were several cases of typhus and typhoid fevers, is a well nourished and generally healthy woman. The cheeks are flushed, conjunctiva slightly suffused, tongue covered with thin fur, and inclined to dry; bowels have been loose the last three days, and were open five times yesterday. She complains of frontal headache, pains in all her limbs, want of sleep, loss of appetite, &c. Has not felt quite well for about a week, but continued at her work until yesterday. She "felt very cold all over" three days ago, and sat over the fire, but could not get warm. Her pulse is 72; temperature, 97° Fah. The next day, and the day after, she exhibited much the same symptoms; her pulse, however, going up to 120 and 100 on the two days respectively, whilst the temperature remained steady between 96° and 97° Fah. Two days afterwards she was quite convalescent, and about the ward again. This case bore as strong a resemblance to that of an early stage of typhoid fever as it was possible to do, and for the first few days many considered it to be one of this disease.

A point of great interest in connexion with this case was the fact that some three months afterwards the patient had well marked typhus, with a temperature of 103° Fah., and a pulse of 120 on the fifth day, which was the first time I had an opportunity of seeing her on this occasion.

4th. That the temperature in every disease has a tendency to run a peculiar course, and has a certain range of altitude, a knowledge of which course and range is of great value as an assistance to us in diagnosis and prognosis.

In connexion with this proposition I should observe that I have not found the temperature in acute disease to be perceptibly affected by the season of the year at which the disease may have occurred, although the temperature of the wards during the year has varied to a considerable extent.

Again, if cases of similar severity at different ages be compared, the altitude attained by the temperature in any disease does not appear to be influenced by the age of the patient, although normally is generally reached somewhat earlier in children than adults.

I have drawn up some charts to show the course of temperature as observed by myself in typhus, typhoid, and scarlet fevers, pneumonia, acute rheumatism, erysipelas, and tonsillitis.

These diagrams are not founded on any average, but merely represent actual typical cases. I have preferred to adopt this plan on account of the fallacies likely to exist in such tables if drawn up from averages. Errors in establishing the exact day of the disease are, in many cases, unavoidable; but these, although only to the extent of a single day, might considerably affect the ultimate result in any general average.

Having discussed typhus and typhoid at some length in another paper I shall not further allude to them. To the other diseases (the course of temperature in which I have endeavoured to illustrate by diagrams) I will briefly refer.

In pneumonia we have high temperatures as early as the second day, with the maximum generally on the third or fourth, and a sudden fall (of perhaps 5° Fah.) to normal on the seventh or eighth.

In acute rheumatism we have a long-continued slightly abnormal temperature, generally ranging between 99° and 102° Fah., and very rarely reaching 103° Fah.

Erysipelas is characterized by very sudden changes of temperature; thus, we often get an alteration of 4° or 5° Fah. in twenty-four hours, and occasionally a fall of 7° or 8° Fah. in the same period. I have observed in several cases that a considerable fall has taken place immediately on the appearance of the characteristic redness.

The temperature of tonsillitis is especially interesting when compared with that of scarlet fever; and as the diagnosis in the early stage between the two is often doubtful, we may frequently be much assisted by the observation of the temperature.

In tonsillitis the temperature in the middle of the day, when at its maximum, which is generally attained about the fourth day, rarely exceeds $100^{\circ}\cdot5$ Fah.; and this occurs with a pulse often below 100. Normal is reached about the sixth or seventh days.

In scarlet fever, on the other hand, the maximum is arrived at about the third day, when the temperature is generally 104° Fah., whilst the pulse is considerably above 100. The normal is not usually attained until the tenth or twelfth days.

Although I have taken a few cases of many other diseases besides those just mentioned, yet I have preferred, at the present time, to confine myself to the examination of the course of temperature in these seven only, considering that my experience of this subject in any other disease has not at present been sufficient for

me to judge fairly of it. I would, however, just allude to the possibility of temperature being of great assistance to us as a means of diagnosis between typhus fever with severe cerebral disturbance (when occurring without any rash), and purely cerebral cases of meningitis or cerebritis. I have only had an opportunity of taking the temperature of two purely cerebral cases, but in both of these the temperature rarely exceeded 101° Fah., whereas with similar symptoms due to typhus fever, we might have expected the thermometer to stand some 3° higher.

I should also state that the highest temperature I have ever taken was $107^{\circ}\cdot 2$ Fah., and occurred in a case of pneumonia following tracheotomy; the next highest was $106^{\circ}\cdot 2$ Fah., in erysipelas; generally, however, temperatures above 105° Fah. have been rarely met with.

I will conclude this part of the subject by bringing forward a case to prove the value of a knowledge of the course and range of temperature in different diseases.

A boy of fifteen was admitted with general *malaise* and sore throat. His tonsils and the back of his pharynx were somewhat inflamed, but his tongue was moist and nearly clean.

After the first few days he complained of nothing except weakness, although his voice continued very husky. His appetite remained pretty good throughout, and he slept well. About the fifteenth day he had some bronchitis, and on the sixteenth some hemorrhage, with a loose motion; previously his bowels had been somewhat confined. The hemorrhage did not recur. There was never any rash or abdominal tenderness, and the physician in charge of the ward considered the patient's illness to be due to the apparent symptoms manifested, viz., the sore throat, with probably some laryngeal mischief, and the thoracic complication.

The course of temperature was that of typhoid fever throughout; and at the time I mentioned this fact to several, but little expected to be afforded any corroboration of my statement, as the lad's temperature fell to normal on the twenty-eighth day; and two days after this he was up and about the ward.

However, as the case was not considered to have been one of typhoid fever, there was no especial necessity to be careful with his diet, and he was placed on meat diet. Having a very hearty appetite he did good justice to the food allowed him, besides partaking of some extras which were surreptitiously conveyed to

him by his friends. The consequence was, to use his own expression, he went to bed "feeling very well," but "very full." Some hours afterwards symptoms of colic came on, which, not being relieved by the appropriate measures adopted, were followed by those of peritonitis of which the lad died on the seventh day from his seizure.

The *post mortem* disclosed a quantity of purulent fluid in the peritoneum, and numerous ulcers of Peyer's glands, the majority of which had already cicatrized, but in many the healing process was still going on. The peritonitis had been caused by an overloaded intestine, previously weakened, the colon being completely stuffed with scybala. There was no perforation.

5th. From the last proposition it follows, that the same altitude of the thermometer attained at one period of any disease is not of the same importance as the same height reached at another time in the same disease.

Thus, in typhoid fever, a temperature which has been rising for two or three days, reaches perhaps 104 Fah. between the seventh and fourteenth days, without causing any anxiety; whereas should the same phenomenon occur about the twenty-eighth day, a fatal termination may probably be expected.

And again, the actual altitude attained on a certain day in one disease is not of the same importance to our prognosis as the same height reached on the same day in another disease. Thus, a temperature of 104° Fah. in erysipelas is very common during the first week, and need not give rise to any alarm; but should such occur at the same date in acute rheumatism I should consider it of much more importance.

6th. That although, in all diseases, a high range of temperature generally indicates a severe case, with a slow convalescence, and a low range usually occurs in a mild case, and is followed by a rapid convalescence; yet there is no actual temperature in any disease which necessarily foretells a fatal termination. Thus I have registered 105°·6 Fah. in a severe case of typhus ending favourably, 106°·3 Fah. in erysipelas, 105°·3 in typhoid; and each of these temperatures was the highest I ever took in the respective diseases.

I certainly once registered a temperature of 107°·2 Fah., in a fatal case of pneumonia; but the great majority of fatal cases generally, although always exhibiting abnormal temperatures prior to collapse, have by no means had any extraordinarily high ranges.

I believe that an abnormal course of temperature is more often the precursor of a fatal termination than any universally high range.

7th. That in the majority of cases a rise of temperature is contemporaneous with a rise of pulse, although such is often not a proportional one, and may not take place at all unless the alteration in temperature be as much as $1\frac{1}{2}^{\circ}$ or 2° Fah.

Cases of typhus are, perhaps, the best examples of the simultaneous alteration of pulse and temperature.

8th. That where the temperature and pulse together do not coincide with the general symptoms, the two former may be generally relied on as to the actual state.

Thus, in typhus, towards its close, we often get a continuous fall of temperature and pulse for perhaps three or four days before there is any improvement in the general symptoms; and in these cases the two former always foretell rightly.

There is, however, a well marked exception to this rule in the later stages of typhoid fever, as mentioned in another paper.

9th. That where the temperature and general symptoms agree together, but do not coincide with the state of the pulse, the two former may generally be relied on as to the actual state.

Thus in cases of hysteria, and in cases of excitable persons approaching convalescence after fever, we often suddenly get a very high pulse, which is sometimes continuous for days, although the temperature and general symptoms are neither of them adverse.

10th. That in those cases in which the pulse and general symptoms remain the same, a moderate fall of temperature on one occasion is not to be relied on; but should such a fall continue in a moderate and gradual manner, for some days, and at such a period when a fall was to have been expected, the temperature may then be depended upon. Severe cases of typhus, towards their close, often give examples of this sort.

In those cases, however, in which the pulse continues frequent, and the general symptoms are severe and without improvement, a considerable fall of temperature (say 5° or 6° Fah.) is to be regarded with anxiety, being probably due to some internal hemorrhage, or to the commencement of a state of collapse.

Thus in a case of icterus, in a girl of fifteen, due to obstruction of the common duct by a hydatid cyst, and accompanied by peritonitis, on the three days preceding death, the pulse had stood at 140, 140, and 136 respectively, the temperature having been on the same days $104^{\circ}\cdot 2$, $104^{\circ}\cdot 5$, and 99° Fah.; whilst the general

symptoms continued without alteration. Here, on the last day, we got a sudden fall of $5\frac{1}{2}^{\circ}$ Fah., which was the first sign of collapse terminating in death some twenty-four hours afterwards.

Again, in a case of typhoid fever, on the seventeenth day there was a sudden fall of 4° Fah. from the preceding day in consequence of considerable internal hemorrhage.

11th. That in those cases in which the pulse and general symptoms continue the same, being the one frequent and the other severe, a continuous rise of temperature for some days, occurring at a period of disease at which some improvement might generally be expected, is usually the precursor of a fatal termination.

Thus, in a case of typhoid fever, terminating fatally on the thirty-sixth day, the pulse and general symptoms continued without change until the thirty-fifth day; the former having stood at 92 on the twenty-ninth day, remained steady and rather below that frequency until the thirty-fifth day, when it suddenly rose to 124. The temperature, however, rose continuously from the twenty-ninth day, nearly a degree a day, until it stood 5° higher on the thirty-fifth than on the twenty-ninth day—the height registered on the thirty-fifth day being 104° Fah.

12th. That although it is possible that the state of the temperature alone in acute disease may, perhaps, hereafter prove to be the one safest symptom to rely upon if taken by it itself (and I believe it is at present, at least, equal to the state of the pulse, and of greater value than this certainly, if only its frequency be taken into account), yet the temperature must be considered merely as an aid, and all other symptoms must be carefully examined into, as it is on comparison with these that its greatest value is always to be found.

The preceding propositions, which are not intended to be put forward as dogmatic rules, being merely the result of a very limited experience, and being probably only approximations to the real truth, which is always deep down “at the bottom of a well,” imperfect though they be, nevertheless contain, I think, amply sufficient to prove the great value of the daily observation of the temperature in acute disease.

I will now refer to those who disregard the value of the clinical thermometer, and amongst these are many of high authority, whose opinion is entitled to great consideration.

What, then, is the reason of the disregard of such? 1st. Some do so because they allege that they can sufficiently distinguish by

their hands normal and abnormal states of temperature, and can carry such sensations in their minds, or commit them to paper under such expressions as "cool," "moderately cool," "warm," "hot," "very hot," "pungently hot," &c.

The fallacy of these expressions can, however, be easily ascertained by any one who will take the trouble, in a few acute cases, first to write down his sensations, and then take the temperature by a thermometer.

Heat and cold are only temperature sensations, and as our hands vary considerably in their temperature, it follows that a body submitted to a hand, the temperature of which is 95° Fah., will refer to the mind a very different sensation to that which it would do were the temperature of the hand 10° or 20° lower.

As an example of this fallacy, I will only mention the following fact:—

I have heard a physician of high standing observe that the skin of a patient was "moderately cool," when the actual temperature in the axilla was 104° Fah. This is by no means an uncommon occurrence. 2nd. There are some authorities who state that an abnormal temperature generally does not occur at all in cases of acute fever, and on this account undervalue the thermometer.

I can, however, honestly state that I have never met with one such case, neither have I found any one who has used a thermometer who has done so, except the patient were in a state of collapse just prior to death.

Consequently, with all due deference to such authorities and their statements, I am strongly inclined to think that the actual existence of such cases is more than doubtful; and if the temperature of a patient which is really 104° Fah. can be called "moderately cool," it will not be difficult to understand how such cases have been supposed to occur.

3rd. There are others, again, who disregard the value of the thermometer because they think they have tried it and found it wanting.

These have tried it certainly, according to their own manner, but generally with utterly wrong and probably self-conceived ideas as to its use. They have started on their trial with the belief that the abstract degree of heat was all that they had to consider. Such as these, consequently, soon find out that a thermometer will lead them into all sorts of blunders, and properly discard it. But would these be merely content with considering the height of frequency to

which the pulse had attained, without taking into account its volume, regularity, its condition yesterday, the disease and the individual in whom it occurred?

Well, then, some very similar conditions must also be considered before placing an actual value on any one observation of temperature—thus, the disease, its period, its height yesterday, &c.; and without reflection on these points, any observation will be useless indeed.

It would, no doubt, save a great deal of trouble if we could decide our prognosis and diagnosis by means of the temperature alone, and also without any practice or experience in the use of the clinical thermometer; but would any one discard the stethoscope merely because some experience in its use was required before any one could be in a position to place a value on the sounds he might hear, or because, even after considerable practice, the value of some sounds (such as certain cardiac murmurs) still remained doubtful?

In the same manner, a person must have some amount of practice with the thermometer before he is in a fit position to put an actual value on any one observation; and any error in diagnosis or prognosis which a beginner may make will be due probably to this wrong interpretation of a certain fact.

The greater his experience the less will be the chance of such an error, and the more valuable will his thermometer be to him.

I will just give two examples of numerous mistakes liable to be made by this class of objectors.

A case of febricula presents itself, and a very high temperature is found within a few hours of the first symptom of *malaise*. The observer in question necessarily thinks that his patient must be suffering from a very severe form of fever, probably typhus, and is proportionally anxious, but in forty-eight hours perhaps, to his delight and astonishment, the temperature is again normal, and the patient convalescent. Such an observer, having considered only the abstract degree of heat, immediately discards his thermometer, although his error of diagnosis and prognosis was due to the wrong interpretation of a fact. Had his experience been greater, the very fact which so alarmed him would have had probably an exactly opposite effect, for he would have known that the disease could not at any rate be typhus fever.

Again, to take an opposite example.

In a case of typhoid fever, in which the general symptoms are severe, the temperature, which has been fluctuating about 103°

Fah., suddenly drops 4° or 5° without any improvement in the general condition of the patient.

Such observers, considering only the abstract degree of heat, regard the altered state of temperature as a favourable sign, and are surprised at, very possibly, finding the patient dead on their next visit. Had they had more experience, they would have known that such a fall was a very unfavourable symptom under the circumstances, being probably caused by intestinal hemorrhage, or else being the commencement of a state of collapse; as in typhoid fever, the general symptoms improve considerably before the temperature reaches normal. All the blame of such unfortunate prognoses is, of course, laid on the thermometer, instead of on their own ignorance and rash judgment.

The above are examples of the misuse of the instrument, in the use of which we must always be very careful not to jump at conclusions, but strive honestly to arrive at the proper interpretation of the additional fact which we have just registered; by so doing we shall, by its assistance, reap an abundant reward in a more successful diagnosis—a more confident prognosis.

In conclusion, I am convinced that the study of the thermometry of disease is as yet in its infancy; and believing that such a study, instead of teaching any disregard for the pulse, or other important symptoms (as some have wrongly supposed it was intended to do), will further enhance their value, and generally conduce to a more attentive examination of each symptom manifested in acute disease, I consider the subject to be one well worthy the attention of every member of our noble profession, and one which will amply repay the few extra moments spent at the bedside.

II.—A CONSIDERATION OF THE COURSE OF TEMPERATURE AND PULSE IN TYPHUS AND TYPHOID FEVERS, AND SOME OF THE POINTS OF DIFFERENCE BETWEEN THESE TWO FEVERS SHOWN THEREBY.

The cases from which the following data were ascertained occurred in St. Bartholomew's Hospital during the last two years, and daily observations were taken of them either by Dr. Warter or myself.

The number of cases amounts to sixty of typhus and thirty of typhoid.

I should here state that in the great majority of these cases only

one observation daily was recorded, but always as near to the same hour (viz., 2, p.m.) as possible.

In consequence of this fact I shall not be able in the present paper to compare the morning and evening temperatures of the two diseases, but will now merely remark, that although the differences between the morning and evening temperatures of typhoid seem to be greater than between those of typhus fever, yet the altitude attained by the thermometer at 2 p.m. has in both diseases appeared to be nearly the mean, between an observation taken at 9 a.m. and one at 9 p.m. I have, however, not had sufficient experience on this particular point to speak with confidence on the subject. I propose in the first place to consider typhus and typhoid fevers separately, and then to examine their points of difference.

Typhus, as met with under the above-mentioned circumstances, seems to occur generally in two forms:—1st. The milder form, the subjects of which are children, or previously healthy adults under thirty, who have led sober and regular lives; 2nd. The more severe form, which has been the one somewhat the more frequently met with. This has occurred either in persons of a more advanced age or in younger adults of unsound condition from previous disease, or in consequence of intemperate habits.

In both of these forms the temperature rises in a more or less regular manner to its maximum, when it falls continuously about a degree a-day until the normal is reached; and although having further fallen below this, it may rise to regain its ordinary height, it will not again exceed this point unless some complication be at hand.

In all cases, also, of either form there is a very uniform temperature of about 104° Fah. on the seventh day; with this exception there is no temperature peculiar to any one day.

In the milder form of the disease the pupils are seldom contracted; the tongue, although generally thickly coated, never exhibits the dry, brownish-black fur of the more severe type; subsultus is not often, floccitatio very rarely present; the delirium (which occurs only at night) is slight, and the rash is not usually very copious. In such the general range of temperature is comparatively low, the termination of the case is, without exception, favourable; and the convalescence, which may be said to begin about the fourteenth day, is rapid.

The temperature attains its maximum on the seventh or eighth days, the average maximum being $103^{\circ}\cdot7$ Fah., the highest

registered $104^{\circ}\cdot3$, and lowest maximum 102° . The normal is reached on the twelfth or thirteenth days.

In the more severe form we often find contracted pupils, a dry, brownish-black tongue, with sordes on lips and teeth, great subsultus and muscular tremors, often floccitatio, delirium frequently present by day as well as night; and towards the termination of the case, a semi-comatose condition often comes on, which occasionally alternates with severe delirium at night. The rash in these cases is generally copious. Under this form I include those exceptional cases of typhus which are attended by considerable diarrhea; these occur in about ten per cent. of all cases, and do not in any other respect differ from ordinary severe cases of the fever.

In the severe form, then, the range of temperature is high, the termination not always favourable, and the convalescence, which does not begin until the end of the third week, is very tedious.

In these cases the maximum temperature is attained between the seventh and tenth days (generally on the seventh or eighth), the average maximum is $104^{\circ}\cdot4$ Fah., highest registered $105^{\circ}\cdot6$ Fah., lowest maximum 103° . Normal is reached between the fifteenth and eighteenth days, both inclusive.

High temperatures, such as 105° Fah. and upwards, do not foretel fatal terminations; but high ranges, generally, at the commencement of cases foreshow severe attacks, and are invariably followed by long convalescences. Fatal cases always exhibit abnormal temperatures prior to collapse, but not necessarily very high ones. Judging from the fatal cases I have witnessed, I believe that an abnormal course of temperature much more generally precedes a fatal termination than any unusually high range.

Such cases have appeared to differ from the ordinary run of severe attacks terminating favourably in one or more of the following ways:—

1st. They exhibit a temperature of only 102° or 103° Fah. about the sixth day, which is below that generally met with even in mild cases; and considering the severity of the symptoms we should expect to find a temperature nearly 2° higher at such a date.

2nd. They have a constant temperature of about 102° Fah. for a week or more.

3rd. They exhibit a fall of perhaps 3 or 4° Fah. on some day on which, in an ordinary severe case, we should not expect any such fall (say on the tenth day).

4th. They have a temperature of about 104° Fah. as late as the

sixteenth day, when even in severe cases the normal ought to be nearly reached.

The pulse generally in typhus rises and falls with the temperature; and between the fourth and ninth days is above 100, often very considerably so; its maximum is reached rather later than the temperature, and normal is attained some two or three days after this point of temperature has been arrived at.

It may, however, especially in persons of nervous temperament, be kept up beyond this date.

In typhus a temperature of $99^{\circ}\cdot5$ Fah. corresponds to a pulse of about 100, a temperature of $100^{\circ}\cdot5$ Fah. with a pulse of 105 and so on; an alteration of five beats of the pulse corresponding to each degree of alteration in the temperature.

In the majority of cases, however, unless the alteration of temperature be upwards of one degree, we shall find, probably, no corresponding increment or decrement in the pulse, and the above only holds good in a general sense.

I have, however, noted down each variation of 2° and upwards in all cases of typhus which I have, in order to ascertain the contemporary behaviour of the pulse; and find that every such variation was attended by a corresponding alteration in the pulse of an average of five beats for each degree. There were only five exceptions to this rule out of the whole number, and in two of these the variation in pulse occurred the next day without any corresponding alteration in temperature.

In concluding these observations on typhus fever I should mention that by far the most frequent complication in these cases was bronchitis, coming on about the twelfth day.

Considerable feebleness of the cardiac systole and impulse was also noticed in several instances.

The fatal cases amounted to about twelve per cent., and occurred either within the first week or else between the twelfth and sixteenth days.

In the former case the deaths seemed to be due to the intensity of the fever poison itself, and in the latter to the circulation in the system of the products of tissue metamorphosis. In only one case, however, were typical uremic convulsions observed. These fatal cases occurred in patients over 40, or in adults above 25 who had led irregular lives. The proportion of male to female deaths in the same number of cases was 7 to 5.

At the *post mortem* examinations of these cases ulceration of

Peyer's glands was never present, and the only peculiar appearance noticed was a dark fluid condition of the blood, and occasionally a dark and abnormally dry condition of the muscles. In one case there was a remarkable effusion of dark fluid blood into the substance of the recti muscles of the abdomen.

The enlarged congested liver and large soft pulpy spleen have been met with both in typhus and typhoid. I will now proceed with the consideration of typhoid fever, which has been met with under two forms of such unequal severity, that I propose for convenience sake to divide the cases as I have done those of typhus, into two classes; one comprising the milder, the other the more severe form.

I should, however, observe that although a severe case of typhus may often be foretold at an early period, yet it is impossible to do so at a similar date in typhoid fever; as I have observed several in which the mildness of the symptoms during the first week or ten days might have augured an early convalescence, eventually turn out of the most protracted character.

In typhoid fever the temperature generally rises, after numerous daily fluctuations, to its maximum; and having attained this point exhibits the same characteristic irregular decline, falling perhaps for two or three consecutive days, and then rising again for two or three more, and so on; the fall, however, preponderating over the rise, until the normal is reached, after which daily fluctuations will probably continue for a few days longer. The majority of cases come under the first mentioned form, ultimately proving of moderate severity, and terminate favourably towards the end of the third week.

In these the pupils are slightly dilated; the tongue, although inclined to dry, and generally abnormally red at the tip and edges, often continues nearly clean throughout. A very common form of tongue which I have observed in such cases has a rather dryish patch, in the form of an isosceles triangle (the base of which is at the tip), running up the centre, with two moist strips of thin white fur on either side. Subsultus and tremors in these cases are rare, and delirium (as often absent as present) is usually very slight, and comes on during the night only. Intestinal hemorrhage does not occur, and thoracic complications of importance are rare.

In these the temperature attains its maximum between the seventh and eleventh days (but this is not reached more frequently on one day than on another); the average maximum is 103·3 Fah.;

highest registered $104^{\circ}\cdot5$, lowest maximum $102^{\circ}\cdot2$ Fah. The normal is arrived at between the fifteenth and twenty-first days.

In the more severe form we generally find that either intestinal hemorrhage occurs during the course of the case, or that we have to deal with a low form of pneumonia, which is a frequent complication, and one which not uncommonly is the immediate cause of death at an advanced period of the fever.

In these protracted cases we get, towards the later stages, dilated pupils, a tongue which is either covered with brownish black fur, and dry, or one which has its central surface glazed, edges and tip red. In both of these transverse furrows may generally be observed. Subsultus and tremors may also be met with, but very rarely floccitatio; delirium, usually of a low form, is pretty constant at night, but rarely present during the day.

In such cases which terminate favourably the temperature reaches its maximum between the nineteenth and twenty-first days, the average maximum is $104^{\circ}\cdot1$ Fah.; highest registered $105^{\circ}\cdot2$, lowest maximum $103^{\circ}\cdot8$ Fah. The normal is reached usually between the twenty-third and thirty-fifth days.

In typhoid fever a high range within the first fourteen days generally foretels a severe and protracted case, but a low range during this period does not necessarily foreshow a mild case, even if the general symptoms be also favourable.

A sudden fall of several degrees, especially if occurring in a severe case, about the end of the second week, and without improvement in the general symptoms, is to be considered with anxiety, such decline being probably due to intestinal hemorrhage, or to the commencement of a state of collapse.

In such an event the pulse, which very likely up to this time may have kept below 100, usually rises suddenly twenty or thirty beats.

In these cases, when the patient does not immediately succumb, the fall in temperature and rise of pulse are frequently followed, within the next twenty-four hours, by opposite phenomena, the temperature rising higher than the point at which it previously stood whilst the pulse falls considerably.

Again, if we meet with a case in which the general symptoms continue severe, but the temperature, although an abnormal one, fluctuates for some days at a point disproportionally low when compared with the general symptoms, we must consider such low range in an unfavourable light rather than the opposite; especially

ought this behaviour of the temperature to be so interpreted if it occur in a case of which the date of the commencement of the fever is unknown or doubtful, and in which the period may then be as late as the end of the third or the fourth week.

Almost without exception in cases which terminate favourably, we find that the general symptoms reach their normal, if I may so speak, several days before the temperature; and although until all fluctuations of the temperature above "the normal" have ceased, the patient cannot be considered safe from liability to a relapse, and great care must be still taken with his diet; yet we may expect in the majority of cases, that if the general symptoms have thus improved we shall have a favourable issue, notwithstanding the slight fluctuation of the temperature above "the normal."

It is the knowledge of this fact that causes us to look with suspicion on cases in which the normal temperature is apparently about to be arrived at before the improvement in the general symptoms. Fatal cases often exhibit a continuous, although often slight, daily rise in temperature for four or more days previous to collapse, and this may occur without any corresponding alteration in the pulse, or without any decided change in the general symptoms.

As we know that a continuous rise for four or more successive days is not met with generally in cases which end favourably, such a rise should put us on our guard.

In typhoid the pulse, to a considerable extent, fluctuates with the temperature, although the exceptions to this rule are much more frequent than in typhus.

However, generally speaking, an alteration of about five beats of the pulse occurs for each degree of alteration in the temperature. Thus, a temperature of $99^{\circ}5$ Fah. will correspond to a pulse of 90; a temperature of $100^{\circ}5$ with a pulse of 95, and so on.

The range of pulses which may be found in different cases, to correspond to the same degree, is, however, very great; thus, between 102 and 103° Fah. I have met with pulses between 80 and 154.

High pulses in typhoid fever do not often occur in adults; and when such are not clearly due to some manifest complication, there seems some reason for believing that tubercular deposit in the lungs, or the general state of tuberculosis, is the cause of this phenomenon.

Fatal cases have occurred in 13 per cent of all cases, and the proportion of male to female deaths, in the same number of patients,

was as 5 to 4. Death took place on the fifteenth, thirty-fifth, and thirty-sixth days, and also in one case on the sixteenth day, of a relapse; besides these cases, the date of death in another patient could not be ascertained, but it was not earlier than the fifteenth day, and very probably took place at a much more advanced period of the disease.

The cause of death was due either to peritonitis, a low form of pneumonia, or asthenia.

In all there was well marked ulceration of Peyer's glands. A low form of pneumonia was the most common complication, and in several cases the fever commenced with some inflammatory affection of the throat.

In all cases diarrhea was all but universally present, at some time in the course of the fever, although in many constipation prevailed for the first fortnight.

Considerable hemorrhage occurred in four cases. Relapses took place in 12 per cent. of all cases. I will now conclude by summing up the differences most frequently observed between these two fevers, as far as their pulses and temperatures are concerned.

1st. The range of temperature is somewhat lower in typhoid, in which, however, a continuous abnormal temperature is of much longer duration.

2nd. The defervescence of typhus is regular and continuous, the temperature falling generally about a degree a day, until the normal is reached; whereas in typhoid, the fluctuations from day to day are considerable, and the fall is not a continuous one daily.

3rd. There is a great tendency in typhus to attain certain temperatures, and to reach certain points in its course, on certain fixed days. Thus, in the great majority of all cases, a temperature of about 104° Fah. is met with on the seventh day; in a very large per centage the maximum is reached on the seventh or eighth days; the normal is generally attained, in mild cases, on the twelfth or thirteenth days; and in more severe ones between the fifteenth and eighteenth days.

In *typhoid* there is no particular temperature generally arrived at on any one day; the maximum may be reached at any time between the seventh and twenty-first days; and normal is attained, in mild cases, between the fifteenth and twenty-first days; whilst, in the more severe ones, it may be attained on any day between the twenty-fourth and thirty-fifth.

4th. The pulse is generally less frequent in typhoid; and although

in both fevers the pulse usually fluctuates with the temperature (about five beats of pulse corresponding to each degree of alteration in temperature), the frequency of the pulse in the two, for the same degree of altitude, is different.

Thus, with a temperature of $100^{\circ}\cdot5$ Fah., we shall get an average pulse of about 105 in typhus, but of only 95 in typhoid, and so on; at each degree the pulse being about ten beats more frequent in typhus than in typhoid. Moreover we sometimes meet with a case of typhoid fever in which the pulse very slightly exceeds normal throughout, but such a case I have not observed in typhus.

5th. In typhus the temperature reaches normal two or three days before the pulse, and the general symptoms often continue severe several days after the pulse and temperature are both normal. In these cases, however, if the defervescence has been regular, and has occurred about the usual period, the case will terminate favourably, notwithstanding the often very unfavourable condition of the patient.

In typhoid, on the other hand, as I have before mentioned, the temperature keeps up several days after the general symptoms have been considerably improved, and we must look, with doubt rather than otherwise, on cases in which this rule appears about to be broken—that is, on cases in which the general symptoms continue severe, although the temperature is but little, if any, above the normal.

6th. Relapses of typhoid have occurred in 12 per cent. of all cases, and in these the temperature ran a similar course, as in the former attack, although at a somewhat lower range.

Relapses of typhus, with a corresponding recurrence of abnormal temperature, have never been met with, although the number of cases of typhus under observation has been double that of typhoid.

The following twelve tables* contain the details of observations on as many selected cases; of temperature, pulse, and respiration, and will serve to illustrate the general method employed:—

* These tables have been deduced from the charts referred to at p. 68.

I. TYPHUS FEVER (severe form). William Birch, aged 21.				III. TYPHOID FEVER (severe form). William Williamson, aged 15.			
Day	Temperature	Pulse	Respiration	Day	Temperature	Pulse	Respiration
8	104.0	114	30	8	103.5	108	36
9	103.1	126	40	9	102.5	100	28
10	102.0	120	40	10	101.5	96	36
11	103.3	128	45	11	101.6	104	32
12	104.0	130	40	12	104.0	100	36
13	103.0	124	40	13	103.0	110	32
14	102.0	100	28	14	101.1	106	30
15	101.3	94	24	15	101.1	114	30
16	99.8	86	22	16	103.5	108	40
17	98.0	84	18	17	105.0	128	36
18	97.6	66	18	18	100.5	106	30
19	96.8	78	20	19	101.2	96	34
20	97.5	62	18	20	100.0	92	32
II. TYPHUS FEVER (mild form). Martha Sheet, aged 18.				21	98.5	94	36
6	102.7	—	—	22	98.3	96	34
7	103.6	141	40	23	98.2	82	30
8	103.8	144	36	24	100.2	94	32
9	102.4	138	40	25	99.8	86	36
10	100.4	126	40	26	98.4	84	34
11	98.8	120	36	27	98.6	76	25
12	97.6	115	36	28	97.5	88	30
13	97.6	108	24	29	100.0	94	36
14	97.6	82	24	30	98.1	78	24

IV. TYPHOID FEVER (mild form). Sarah Langton, aged 14.				VI. SCARLET FEVER. Alice Kirke, aged 5.			
Day	Temperature	Pulse	Respiration	Day	Temperature	Pulse	Respiration
8	102.2	114	—	3	104.2	162	—
9	101.2	105	—	4	104.1	147	—
10	102.1	101	—	5	101.2	122	—
11	101.3	99	—	6	100.0	106	—
12	101.3	126	—	7	99.1	120	—
13	100.0	93	—	8	99.1	120	—
14	101.4	111	—	9	99.6	118	—
15	101.9	117	—	10	99.8	102	—
16	99.0	108	—	VII. FACIAL ERYSIPELAS (mild). Caroline Phillips, adult.			
17	99.9	99	—	3	99.2	102	21
18	98.2	114	—	4	102.9	104	24
19	100.4	120	—	5	99.7	114	32
20	96.0	81	—	6	103.2	120	32
21	98.5	93	—	7	99.0	96	32
V. SCARLET FEVER. William Howell, aged 3½.				8	100.0	96	24
4	103.5	144	30	9	97.3	90	32
5	103.0	160	30	10	96.5	71	24
6	100.3	120	32	11	94.2	81	32
7	99.0	114	26	12	96.0	75	28
8	98.5	118	26	13	95.3	57	32
9	98.9	118	24	14	96.2	54	24
10	98.3	—	—	15	95.3	51	24

VIII. FACIAL ERYSIPELAS (severe).				X. TONSILLITIS. Sarah Baker, aged 16.			
Day	Temperature	Pulse	Respiration	Day	Temperature	Pulse	Respiration
6	104.3	108	26	3	100.7	120	30
7	105.2	116	24	4	101.0	122	25
8	98.2	72	20	5	100.0	112	28
9	100.0	105	24	6	98.3	94	22
11	94.0	82	16	XI. PNEUMONIA. Sarah Baker, aged 16.			
12	101.5	102	16	2	102.8	120	48
13	97.0	90	24	3	104.8	122	42
14	97.2	75	20	4	104.0	136	48
15	95.8	56	16	5	104.2	136	56
16	97.8	78	24	6	98.7	96	42
17	96.2	66	24	7	98.2	88	36
18	99.9	87	24	8	95.8	80	36
19	96.0	60	24	XII. ACUTE RHEUMATISM. Female, aged 34.			
20	96.9	62	20	8	101.7	108	18
21	95.9	—	—	9	100.8	108	20
22	96.2	—	—	10	100.7	100	20
23	95.8	—	—	12	101.5	100	20
IX. TONSILLITIS. Eliza Halse, aged 13.				13	101.3	100	20
4	100.0	92	20	14	101.1	94	22
5	99.2	80	20	15	100.5	96	22
6	98.7	88	24	16	100.6	96	22
7	97.5	82	24	17	100.2	108	22

ART. IV.—*The Medical Uses of Chloroform Inhalation.* By
CHARLES KIDD, M.D.; M.R.C.S.E., London.

THE medical uses of chloroform, or rather its administration in medical as contrasted with surgical cases, begins to assume a form of very considerable interest and importance to the practising physician—not so much as an agent capable of removing slight or severe pain in neuralgic or purely spasmodic affections, as in controlling dangerous convulsions in the adult or infant, as a remedy also, or auxiliary of great usefulness in various forms of epilepsy, though not so effectual in simple hysteria, chorea, or delirium tremens. The medical uses of chloroform, in a word, have not been recognized sufficiently; its far wider and more brilliant aid to the operating surgeon having somewhat eclipsed its occasional applicability in the less demonstrative clinical wards of the physician. In cases of puerperal convulsions—which come under the notice of the general practitioner or young obstetric physician—the varied experience of all, or of the best obstetricians, agrees that whether we have albumen in the urine or not, the careful, skilful administration of chloroform, even before adoption of the lancet, seldom fails to afford marked relief, and that in a majority of cases we may dispense with the excessive venesections of former times; chloroform in such cases, of course, is not to supersede other means usually had recourse to, such as delivery, &c.

In cases of severe agony, and jaundice attending the passage of *gall-stones*, the inhalation of a drachm or two of chloroform will often act like a sudden charm in affording relief from pain and spasm. It is probable here, that there is produced a relaxation of the ductus choledicus, and associated muscular or nervous fibres of the adjacent diaphragm, duodenum, abdominal muscles, &c., all thrown into spasm by an irritating calculus; it is curious, too, that jaundice has been occasionally produced by chloroform, especially when given in small or irritant doses; but this is a much more rare phenomenon than the cure of jaundice by it.

A case of the following kind has come under notice:—A gentleman in the higher circles of society, an old gentleman subject to bad attacks of jaundice and gall-stone, in the country, was supposed to be at the point of death from “black jaundice” and impacted gall-stones; an entire week of horrible agony had passed; warm baths, blisters, purgatives, globulistic remedies, ipecacuanha,

and every other conceivable remedy had been tried in vain. Opium had made things worse, and emetics no better; a fair trial, in one word, was given, or supposed to be given, to the good old classic abracadabra of Copeland and Watson, that

“ Old Experience which doth attain
To something of prophetic strain.”

Experience of all kinds had been summoned from the nearest country town; but with amiable suavity, Old Experience had pronounced the case incurable; it had been well excogitated that the bile secreted by the hepatic cells had probably regurgitated along the cystic duct into the gall-bladder; there stored up it became concentrated; how cholesterine (not Dr. Thudicum's blood corpuscles) had formed a calculus, with much more, all pointing to sadly established organic disease of the worst kind. In this emergency, as a matter of “ridiculous form,” as it was said by Old Experience, a young but eminent city physician was summoned by telegraph—a man of the new school, eclectic and sensible, who are glad to adopt remedies from whatever side of the compass they come.

“Have you tried chloroform,” he ventured to ask in consultation? “Nothing like leather—of course not; but perhaps you would like to do so.” To which he replied in the affirmative.

A few drops of chloroform, in fine, were tried, with the best possible result; the agony of the gall-stone yielded as if by some potent spell, the simple *rationale* of the cure being that it relaxed the muscular fibres, perhaps, of the pylorus and duodenum, the gall-ducts, diaphragm, &c., and allowed the gall-stone to pass, whereas previous purgatives, blisters, opium, &c., had only tended to derange the healthy sympathy which usually exists between the mucous membrane of the duodenum and the bile passages. In a sad case of this kind, not very long ago, it will be remembered, one of our most eminent members of Parliament died of the agony of gall-stones, and the derangement brought on by large doses of opium; it is probable one drachm of chloroform would have saved his life.

In some of the worst sufferings of uncomplicated asthma and in hooping-cough I have known the inhalation of chloroform to prove very beneficial; indeed it is clear that the often-vaunted popular cures of hooping cough, by children inhaling the vapour at gas works, is easily and only to be explained by the calming influence on the glottis of some of the many gaseous carbo-hydrogens

belonging to coal-gas, so like chloroform. I have known also the popular specific, or liniment of oil of amber, camphor, &c., for hooping cough, to prove far more useful when chloroform and ether were added to the formula—the liniment rubbed to the chest and neck, rather than in the time-honoured method, to the spine of the child. If hooping cough (as it often is) happens to be mixed up with bronchitis, or if asthma be associated with emphysema, the chloroform will cause disappointment if it does not aggravate matters. A little antimony for the bronchitis may, however, be combined with the inhalation method, and will never disappoint in the bronchitis of children; the direct application of solution of nitrate of silver to the glottis, in hooping cough, is also facilitated by the use of chloroform.

Again, in various forms of epilepsy, though theory at first contraindicated the administration of anesthetics, subsequent experience has fully established the fact, that, contrary to presupposed views, chloroform in full doses controls the epileptic fit; and while good breathing is kept up there is no cause of apnea, or alarm from suffocation. It is however as an auxiliary—in allowing a full examination as to the peripheral origin of the epilepsy, and surgical interference, by caustic or knife, leading to the cure of hundreds of such cases—that chloroform has remodelled the entire practice of epileptic diseases; the mechanism of the epileptic fit has now been so well explained by Brown-Séquard, Reynolds, and others, and the fit itself so often found to depend on external or peripheral sources; for instance, irritation of bad teeth, worms, external excitement of the genito-urinary organs, that a trial of chloroform, especially in female epileptics, seldom fails to yield suggestive results. Chloroform, on the other hand, is of less value (if not attended with positive risk) in hysteria and chorea; it is also of questionable safety in the debility of delirium tremens and puerperal mania; but in the other affections detailed—convulsions, asthma, hooping cough, gall-stones, obstetric practice generally, and many other purely medical cases—it will seldom fail to give most beneficial results.

In these observations I have not dwelt on the use of chloroform, except by inhalation. Every one, however, now knows the great usefulness of “chlorodyne” in all the several varieties of which Roscoe and others have shown that chloroform is the active agent, though at first denied.

ART. V.—*Diphtheria ; a Paper Read before the Medical Society of the College of Physicians, Ireland, March 21, 1866.* By DR. HAYDEN, Physician to the Mater Misericordia Hospital.

ALTHOUGH the name diphtheria can, with strict propriety of language, be applied only to such cases as present, amongst other characteristic symptoms, that of pellicular exudation in the fauces or pharynx, still, cases are not unfrequently met with during an epidemic visitation of diphtheria, and even sporadically, which, presenting most of the usual features of genuine diphtheria, including vascular engorgement of the throat, the absence of false membrane would scarcely warrant us in excluding from a category the members of which we learn to recognize rather by generic than specific characters. The appearance of diphtheritic exudation being, moreover, usually, if not invariably preceded by certain general and local symptoms, the disease may not proceed beyond the first stage, in which the throat is simply inflamed, either because of the decline of the epidemic influence, or of the constitutional resistance of the patient, or, perhaps, of the early and successful application of remedies.

M. Empis says:—"From the circumstance that diphtherite is a phlegmasia, it should at first present the phenomena which are common to every inflammation; and this is, in fact, consistent with my observations, and all my cases testify to the truth of this view."

A few of the cases which I am about to adduce presented diphtheritic exudation in only minute patches, and in another there was no exudation on the occasion of my visit, although it ultimately proved fatal.

The following cases are reported by Dr. Moore, of Amiens-street, with whom I saw the patients:—

On Tuesday, 18th April, I was called to Clontarf, to see a child of Mrs. K's. Several cases of diphtheria having been reported in the neighbourhood, and one of her own children having died on the Saturday previous (15th April), she determined to take her three children to Dublin for some time. She left the country on Sunday evening; her eldest child, a girl of eight and a-half years, complained of headache, &c., on Monday evening, and I was requested to see her on the following day: she is a rather delicate child; when I saw her she was feverish, skin very hot, respiration easily performed, face flushed, could swallow very fairly.

On examining the fauces I found a patch of slough over the left tonsil, about the size of a shilling, raised, and pale yellowish in colour. I touched the surface of the slough with tincture of the sesquichloride of iron, and directed a mixture containing hyposulphite of soda and compound tincture of cinchona, and also a powder containing half a grain of James's powder and calomel, to be taken every third hour. I advised Mr. K. to remove the child into town, as being more convenient for treatment, and also to separate her from the other children in the same house. I saw the child a second time this day when removed into an hotel, and again applied tinct. of the sesquichloride of iron, and ordered nourishment: beef-tea, arrowroot, &c.

On the 19th I had the advice of Dr. Hayden, who continued daily to see the child with me; he directed the hyposulphite to be continued, but in less quantity; five-grain doses being now given every third hour in same vehicle as before. At his suggestion I now commenced to apply, thrice daily, to the slough, which had spread to both tonsils and uvula, a lotion containing chlorate of potash and tinct. of myrrh; a small blister was placed under each angle of the inferior maxilla.

20th.—She swallowed pretty well, and has had almost no difficulty in this respect; the blisters did well; continued the application to the throat; some griping and tenesmus produced by the hyposulphite; the same mixture continued, with the addition of $\mathfrak{z}\text{ii}$. of syrup of white poppies.

21st.—A stimulating liniment to be applied over throat, sprinkled on lint, composed of tinct. cantharid, with compound spirit of lavender, and soap liniment; nourishment and other treatment to be continued; she sleeps fairly.

22nd.—Continue mixture, &c.; some tendency to diarrhea, for which were prescribed six grains of compound powder of chalk every third hour; sloughs are being detached; liquor of the pernitrate of iron ordered to be used should bleeding come on; the patches of ulceration look much smaller and more healthy to day.

23rd.—Continue treatment; give chicken jelly, &c.; has been taking wine for some days; pulse has been generally about 120.

24th.—Bowels not free; ordered aperient powder and oil draught.

25th.—Bowels free; ordered to take five-grain doses of hyposulphite of soda in half an ounce of infusion of cinchona every third hour; nourishment, &c.

27th.—Improving very much; ordered quinine, one-grain dose three times a day. On the morning of this day the youngest of the children, a boy about two and a-half years, was brought to me; he was feverish, face flushed, tonsil exhibited ulceration, but to a much less extent than in the girl; it did not spread so much. Dr. Hayden ordered her to have half a drachm of wine of ipecacuanha every fifteen minutes until emesis was produced; afterwards an aperient powder, and three-grain doses of hyposulphite of soda in half an ounce of infusion of cinchona every third hour; the same form of application to tonsil, and nourishment.

28th.—Both children doing well.

29th.—Youngest to have small blister under angle of jaw.

30th.—Youngest directed to continue hyposulphite of soda.

May 1st.—A blister over tonsil; same treatment as before.

May 3rd.—Slight diarrhea—arrested by five-grain doses of compound powder of chalk; from this date until the 8th improvement in both children.

I have seen the eldest some time since in good health; the uvula, which was partially destroyed by slough, is shorter than natural.

M. L., a pawnbroker's assistant, aged 30, who had been repeatedly, during the last two years, under my care for various ailments, chiefly of a nervous character, consulted me about 6th of November, 1865, chiefly in reference to severe night sweats and great debility; his pulse was weak and rapid, tongue foul, there was general palor, loss of appetite and sleep, and rather troublesome cough, with mucous expectoration. On careful examination of the chest, the only physical evidence discovered, calculated to warrant the apprehension of tubercular deposition in the lungs, was a loud soft systolic murmur, heard beneath the left clavicle at the end of inspiration, and manifestly having its seat in the subclavian artery; percussion was everywhere normal, as were likewise the heart's action and sounds.

As the patient had been much confined in a close ill-ventilated shop, and was of strumous appearance, I confess that in the absence of any other rational explanation of the severity of his night sweats, which were such as to saturate two shirts each night, coming on only towards morning, and after he went to sleep, and of his remarkable debility after an illness of only three week's duration, I was disposed to regard the case as one of acute phthisis, with incipient but rapid tuberculization of the left lung; the murmur in

the left subclavian artery being due to the ^{sure}presence of the tuberculized apex of the corresponding lung upon that vessel at the acme of inspiration.

The treatment consisted in the application of a small blister under the left clavicle, with some expectorant and astringent medicine.

The patient visited me again in the course of a few days, not at all improved either in his general condition or as regarded his urgent symptoms.

I now directed a change of residence, and prescribed small doses of cod-liver oil, with syrup of the iodide of iron, and continued to visit the patient at his lodgings; no improvement, however, took place; the night sweats continued, rendering necessary a change of linen twice in the course of each night, and debility increased; whilst, at my morning visit, the pulse was rarely above 96, and the subclavian murmur had latterly vanished. I felt greatly puzzled, and considered whether the case might not be one of irregular intermittent fever, due to bad ventilation and drainage, both of which conditions were present where the patient was in the habit of sleeping when at home; two-grain doses of quinine were accordingly prescribed, but with no better result than before: the perspirations were, however, checked by sponging the body over with tepid vinegar and water. Up to this time (Nov. 18) the patient complained of no soreness of the throat, no difficulty in swallowing; this morning, however, he "hawked" up rather significantly, and complained of slight uneasiness under the left angle of the inferior maxilla.

On examining his throat I found the entire soft palate of a deep dark red colour, dotted over with patches of dirty looking fawn-coloured pellicle; the left tonsil was similarly, but more extensively covered, and the isthmus and back of the pharynx were occupied by strings and bands of viscid mucus.

The nature of the case was now no longer doubtful; it was an example of diphtheria, of a novel and anomalous character; novel in its positive and negative symptoms, viz., in the presence of profuse colliquative night sweats, with intervals of all but complete apyrexia, in the absence of the characteristic sore throat, until five weeks after the date of first illness. A more minute inquiry now revealed the fact that the patient had been visiting a fellow assistant in the establishment, who had been lying ill of "fever" of some kind, in one of the upper apartments of the house, and that about that time

he felt a chill after returning home from business, late one evening; he slept indifferently that night, and was sick, unable to take food, and generally unwell next morning; in the course of a few days perspiration set in; he continued at business, though scarcely able to stand, for three weeks after this date, and up to the time of his first visit to me. I directed an eight-ounce mixture containing three drachms of hyposulphite of soda, with syrup of poppies and compound tincture of cardamoms, of which he was to take a tablespoonful every third hour, and also ordered a gargle of biborate of soda and chlorate of potash; six ounces of wine in the course of the day, beef-tea with bread-crumbs, *ad libitum*.

November 20th.—Generally improved; sweats less.

November 25th.—Patient much improved as regards appetite, sleep, and strength; sweating still continues, but in less quantity; throat quite well. About this time I recommended the patient to go to the country for change of air, and lost sight of him.

John J. T., aged 35, was admitted into hospital January 28th, 1865. Had been a clerk in a close office, and lived in a dark ill-ventilated room. Had diphtheria six weeks ago, for which he was treated in another hospital. Throat and fauces dry, red, and granular-looking, with a few white dots dispersed at intervals; patient is only half conscious, and is profoundly deaf; was slightly deaf before present illness; is remarkably irritable; pulse regular.

February 2nd.—Pulse 60, and weak; every third or fourth beat of heart, as likewise of radial pulse, is lost; no cardiac murmur, or derangement of respiration. To have five-grain doses of chlorate of potash with chloric ether, in infusion of cinchona, three times a day; a gargle containing tinct. of myrrh; nourishing diet, and wine.

February 3rd.—Intermission of heart and pulse about once in 60 beats; pulse 90, and of better volume; first sound of heart dull and somewhat muffled. To have one pint of porter and eight ounces of wine daily.

February 4th.—Pulse 42; every second beat lost, as likewise of heart. Ordered eleven grains of hyposulphite of soda, with compound tinct. of cardamoms, three times a day.

February 7th.—Intermission of heart irregular; action of heart and pulse exceedingly weak; pulse 42; tongue moist, and aspect of patient manifestly improved.

February 8th.—Pulse rather stronger, and intermitting regularly; i.e., once in fifteen beats, the rate being 84 in the minute, whilst in

the succeeding minute every alternate beat was lost *four times in succession*—the rate being also slower, so that there were only 48 beats in the minute; patient generally improved; tongue moist and clean; appetite good; allowed to get out of bed to-day at his own request, as bed-sores were threatened.

February 19th.—Deafness much improved; tongue clean, and pulse no longer intermitting; appetite good; a small abscess formed in the right external auditory meatus. Diarrhea, which is attributed to hyposulphite—latter consequently stopped, and small doses of Dover's powder given. From this date till the 1st of March, when the patient was discharged from hospital perfectly well, the progress towards recovery was uninterrupted.

Of the two following cases my notes are very meagre, as the patients were seen in the dispensary, and I had time only to record a few particulars respecting them.

William W., aged fourteen, felt ill on the 5th of May. I saw him on the 8th, when the throat was inflamed, and covered with patches of false membrane; ordered hyposulphite of soda in five-grain doses, three times a day. Seen again on the 10th, and also on the 15th, when he was dismissed as cured.

James W., aged sixteen, brother to the last patient, was taken ill on the 16th May. I saw him on the 17th, when the pharynx was red, and both tonsils were covered with pellicular exudation; ordered hyposulphite of soda in six-grain doses thrice daily. I lost sight of this patient, and therefore cannot speak positively as to the result of treatment, but as there was no further report of him I presume his case went on favourably.

Mr. M., aged seventy, residing in a well-ventilated house, on an elevated site, complained of a sensation of chill, with debility and soreness of throat, towards the end of last January. I saw him, with Mr. M'Crea, of Clondalkin, on the 4th February: the fauces and pharynx were then of a deep-crimson colour, without exudation; there was engorgement of the bases of both lungs, and slight cough; pulse quick and weak; tongue dry, and surface cool. The treatment consisted in sulphate of quinine, brandy, strong broths, and a gargle containing tincture of myrrh. In this case, owing to irritability of stomach, and the absence of exudation in the pharynx,

no specific treatment was instituted. In a letter which I received from Dr. M'Crea, on the 15th of March, he says—"His throat continued a long time relaxed, but slowly improved as his general health got better; he got on so well for a full fortnight as to take broths, and wine, and a chop every day; he was able to sit up the greater part of each day, until he unfortunately went down stairs and caught cold; got a fresh attack of his former symptoms, and sank very rapidly, though plenty of nutriment and stimulants was given."

James K., aged forty, porter, admitted into hospital January 15th, 1866; complained a few days previously of headache, shivering, sickness of stomach, and pricking of throat; pulse quick and weak; throat dark red, with minute patches of false membrane of a dusky-yellow colour dispersed over the tonsils and palate; there is loss of appetite and somnolence; to have ten-grain doses of hyposulphite of soda, with syrup of poppies, in camphor mixture, every third hour; a gargle composed of chlorate of potash and tincture of myrrh; six ounces of wine daily, and beef-tea.

On the following day this patient was attacked with erysipelas of the right side of the head and face, which was treated locally with sulphate of iron in the form of ointment; he was altogether thirteen days in hospital, during which there was no alteration in the treatment, except in the substitution of quinine for the hyposulphite, a few days before the patient left hospital; he was discharged on the 28th January, quite restored to health. The occurrence of erysipelas in this case is noteworthy; Dr. Greenhow remarks:—"Erysipelas has very often coincided in prevalence with diphtheria."

As my object in bringing forward these cases was only to illustrate the remedial action of the hyposulphites, in the hope that others may give these agents a more extended trial in the treatment of diphtheria, I will discuss the general pathology of the disease only so far as it seems necessary, in order to justify what may be called specific treatment. Dr. Jenner declares that "diphtheria is a general disease—runs a quick and definite course, and has a specific cause." In this opinion I entirely concur; and, indeed, it will be difficult to controvert it, if due weight be given to the epidemic character of diphtheria—the great debility which accompanies, and frequently succeeds it—the frequent occurrence of albuminuria, and of specific exudation upon abraded surfaces, and lastly, the remarkable lesions of motion and sensation which are occasionally met with amongst the sequelæ of the disease.

If diphtheria, then, be a general disease, manifesting itself by constitutional as well as by local symptoms, it follows that general treatment is indicated; and further, if we admit that it is a specific disease, we are, as it seems to me, warranted in searching for a specific remedy. The remarkable experiments of Dr. Polli, of Milan, who has succeeded in arresting putrefactive fermentation by means of the hyposulphites of the earths and alkalies, encourage the hope that zymotic or catalytic diseases may be controlled by the same agents administered medicinally. It is true that both Bretonneau and Trousseau declare they have no confidence in the general treatment of diphtheria; but medicine is a progressive science, and we should not regard as final the adjudication of even the most eminent of our brethren upon a question still open to investigation.

In *The Dublin Quarterly Journal of Medicine* for August, 1864, Dr. De Ricci has published some cases of septicemia successfully treated with bisulphite of soda. Encouraged by these results, as well as by several cases of diphtheria which recovered under the hyposulphite treatment, reported from time to time in the journals, I determined to give these agents a trial; and although some of my cases were examples of the milder form of the disease, or the "common membranous angina" of Bretonneau, others, as Cases I., II., and IV., were sufficiently serious to warrant an unfavourable prognosis. Death occurred only in one instance amongst the eight cases; in that instance alone the hyposulphite was not given; the patient was seventy years of age, and his case was complicated with congestion of both lungs.

I do not mean to assert that in the hyposulphites we have got an agent capable of neutralizing or decomposing the toxemic principle of diphtheria, whatever that may be; but of their curative properties in this disease I think we have sufficient evidence to warrant us in giving them a more extended trial. The only inconvenience which I have experienced in the use of the hyposulphite of soda was the occasional occurrence of diarrhea, but this was readily corrected by the addition of a little syrup of poppies, or other mild astringent.

ART. VI.—*Reports of Hospital Cases:—Strangulated Inguinal Hernia.* By WILLIAM MAC CORMAC, M.A., M.D., Q.U.I., F.R.C.S.I.; Surgeon, Belfast General Hospital.

UPON such a subject as hernia it would, perhaps, be difficult to bring forward anything very novel, yet so various and so unexpected are the peculiarities occasionally presented, that an account of the four following cases will not, I should hope, prove uninteresting. All four were examples of oblique inguinal hernia; two being of the kind termed congenital, and two of ordinary inguino-scrotal hernia. These cases serve to illustrate the diversity which even one species of rupture may present; a circumstance which calls forth the resources of the surgeon, and gives practical interest to the faithful record of almost every case of this always dangerous, and not very infrequently fatal, disease.

Inguinal hernia we know to be the commonest form of rupture. About one-half the cases of strangulated hernia are of this kind; but of these, according to Mr. Bryant's statistics, one only in three requires herniotomy, the other two being reducible by the taxis. In all the four cases, the particulars of which I am about to relate, the taxis proved unsuccessful, and an operation was found necessary.

The skill of the surgeon requires mainly to be directed to solving the difficult problem as to the precise time when operative interference becomes imperative, and milder measures should be abandoned. When that decision has been arrived at, the more promptly it is carried into execution, both for preservation of life and the credit of surgery, the better. Vidal, in his great surgical work, puts this matter very forcibly, as in the following words:—"Je ne crains pas de le dire, c'est aux temporisations, et aux manœuvres trop répétées du taxis, qu'on doit rapporter les neuf dixièmes des succès de la kélotomie." And our own Mr. Hey tells us:—"He had often occasion to lament that he had performed the operation too late, but never that he had performed it too soon." Indeed, all modern experience proves to us with what impunity the peritoneal cavity may be opened. It is not, I believe, wounding the peritoneal sac which we need so much dread as the devitalizing effects of long-continued strangulation of its contents; and, of course, the longer this condition is permitted to subsist the greater the fatality after operation must prove.

Of all the auxiliary means employed to reduce strangulated

inguinal hernia chloroform is admittedly the most valuable, and the question arises whether it might not be desirable to use it primarily in all cases. Mr. Bryant is very strongly in favour of our doing so; and he states, in support of his opinion, that amongst seventy-eight instances of reduction by the taxis of strangulated inguinal hernia, he found fifteen, or nearly twenty, per cent. were completed under the influence of chloroform, and that in nearly all these instances all other means had been attempted, and had failed; reduction being effected by the surgeon who had been summoned to operate. It appeared to him to be only a natural and fair conclusion that we should primarily employ the means which have been proved so successful, and not waste time by the employment of other remedies when we possess one so incomparably superior. It was not possible to carry out this plan in treating all the following cases, but I am sure it would be the best rule to adopt in treating the very acute forms of this disease.

I now propose briefly to describe the examples of strangulated hernia which form the subject of this communication, and to point out what appears to me chiefly interesting in each:—

The first case was that of a man twenty-seven years of age, a chain-maker by trade. He was admitted to the Belfast hospital between twelve and one o'clock on the morning of the 6th November, 1864, suffering from a strangulated inguinal hernia. He stated that he had been subject to rupture on the right side since childhood, but only wore a truss for the last two years. The instrument had lately been broken, and failing to get it replaced, the gut was continually coming down; and he often found great difficulty in replacing it. Last night, the 5th, at half-past seven o'clock, the hernial protrusion suddenly made its appearance, apparently after no unusual effort. Being quite unable to reduce the swelling, and getting alarmed at the pain and sickness he began to experience, he obtained admission into hospital.

On examining the patient, about five hours after the accident, I found the right side of the scrotum distended by a very large tense and elastic tumour, cylindrical in form, with rounded ends. The swelling measured fully ten inches in length, and fifteen inches in circumference, as nearly as could be calculated. The penis was invisible. The testicle could be felt at the lower and back part of the tumour, gliding freely over its surface, and evidently contained in a distinct sac. Hardly any impulse could be detected upon

coughing. Twice, before admission, the patient had vomited bilious matter, and he now complained of great nausea and depression, and pain in the pit of the stomach. There was very little local tenderness or pain complained of. I diagnosed a hernia into the vaginal process of peritoneum, with the testicle shut off in its own proper tunic. The name given by Mr. Birkett to this form of congenital hernia, which was first recognized by Malgaigne, is hernia into the funicular portion of the vaginal process of peritoneum, which describes more correctly than any of the other terms employed the particular form which the disease assumes in these cases. My principal reason for supposing the hernia to be of this form was its existence from childhood, from a date long anterior to the patient's recollection; for, as Pott observes, the appearance of a hernia in very early infancy will always make it probable that it is congenital. This variety is now known to be of nearly as frequent occurrence as the ordinary congenital hernia described by Haller. Immediately after seeing the patient I gave him a dose of opium, and had him placed in a warm bath. On trying the taxis in the bath I was able in a very short time to reduce the tumour to two-thirds its former size, and I expected that in a short time the entire protrusion would disappear within the abdomen. I was disappointed, however, for no efforts on my part made any further change in the bulk of the swelling. The patient was, nevertheless, much relieved, and I felt justified in postponing any further attempt at reduction until morning. I requested my colleagues to meet me at 10 a.m. At that hour, on visiting the patient, I found that he had slept well. He complained of very little pain or uneasiness. The pulse was 60, and there seemed to be no urgent symptoms. After the consultation, at the instance of one of my colleagues who was present, I ordered the application of ice bags to the part, and the administration of a fetid enema. My own inclination was, however, immediately to attempt reduction, under chloroform; should it fail, to operate.

The following day I found that the man had spent a restless night, and had vomited several times. He now complained of constant nausea, and severe pain in the neck of the tumour, on which the remedies employed seemed to have produced no effect. The taxis, conjoined with the trial of the other means before-mentioned, having failed to effect more than a partial reduction of the tumour, herniotomy seemed the only alternative left, should the taxis, under chloroform, fail. In this view my colleague, Dr. Murney, fully concurred. The man was, therefore, anesthetized,

and the taxis tried, both by Dr. Murney and myself. It failed to produce any further diminution in the bulk of the swelling. Accordingly, I proceeded to make an incision, three inches in length, in the axis of the tumour, from a point over the internal ring, downwards to the scrotum. The different layers of fascia were carefully divided upon a director, and the sac exposed. Across it, at the region of the external ring, stretched two or three fibrous bands. These were, one after the other, divided, but the hernia still remained irreducible. On a search been made for the cause, a thick fibrous band was found incorporated, apparently, with the neck of the sac. This was most cautiously divided; and, in so doing, the sac, of which it seemed to form an integral part, was opened. A quantity of dark red serum spouted out a yard from the operating table, giving those present the impression that a hydrocele was being operated upon in mistake for a hernia. The quantity of fluid was enormous, amounting, according to our estimate, to between thirty and forty ounces. Projecting from the inguinal canal were now seen a knuckle of small intestine, dark red in colour, and much congested, and a small piece of omentum. That the stricture was a very tight one was proved by the condition of the intestine, and from the fact that the fluid in the hernial sac could not be forced through it. No difficulty was now experienced in reducing the protrusion. After it had been returned a large additional portion of fluid flowed away from the peritoneal cavity. Four sutures were inserted into the wound, and water dressing was applied. The after progress of the case was uniformly good. The pulse never rose above sixty-four. On the sixth day after the operation the bowels were opened naturally, and on December 12th, rather more than one month after, the report was as follows:—"The wound is now completely healed, and the patient is walking about the ward." He was discharged soon afterwards, wearing a double truss, which was rendered necessary by the weakness of the abdominal wall on the left side. The side operated upon appeared much the stronger of the two.

This case was unusual, both on account of the large size of the tumour and the nature of its contents. It might have been mistaken for hydrocele, as the swelling had quite the same rounded form; the impulse on coughing was not at all well marked, and the neck of the sac was comparatively small. The history and the symptoms of the case, however, precluded this idea. Another feature of interest in the case was the partial reduction of the swelling, and

the subsequent impossibility, by any of the means employed, of making a further diminution in its size. In congenital hernia, reduction by the taxis is always difficult, since the inguinal canal is unshortened, and the stricture is generally tighter, and symptoms of strangulation occur earlier and more severely than in the acquired form of hernia. Although no untoward result followed, it is to me a matter of regret that the operation in this case was not earlier performed. It is, indeed, unfortunate when any unnecessary delay takes place, after a distinct failure to return the protrusion by some of the ordinary methods, as the possible advantages cannot, I believe, countervail the certain disadvantages which such a line of practice is calculated to entail.

The second case was that of a telegraph clerk, twenty-two years of age, a person of considerable intelligence. He was admitted to hospital on the 26th May, 1865, at ten o'clock in the evening, with a swelling in the right side of the scrotum, which had been ascribed to an enlarged testicle by his medical attendant. The true nature of the case was not recognized upon admission. It was supposed to be orchitis. I was not sent for; and until my ordinary visiting hour, ten next morning, I did not see the patient.

The history of the case is as follows:—The man stated that for a longer period than he could recollect—he believes from infancy—he had been subject to the frequent appearance of a swelling in the scrotum. The first occasion on which he remembers noticing it was when a boy of ten, while playing at ball, a large tumour suddenly appeared in the scrotum. On assuming the horizontal posture it vanished. The patient all the time, however, believed it was his testicle which was affected. He is intelligent, and appeared to know quite well what was meant by a rupture, but felt positive that he never had had anything of the sort. He had never worn a truss. Three weeks since the swelling suddenly showed itself in the scrotum, and he found himself unable, as before, to cause it to disappear. Believing the testicle had become inflamed he took some purgative pills and a seidlitz powder. Finding himself getting worse, and suffering from sickness, prostration, and hiccough, he obtained admission, on the 5th May, to a county infirmary. There, the patient's own statement as to the nature of his disease, was apparently adopted as the correct one—if the treatment he received is to be considered any criterion. Six leeches were applied to the part, and hot stupes; some pills were also given to purge him, followed in the morning by a black draught. On the fourth day after his admission the swelling, he states, suddenly

and spontaneously disappeared; the bowels were then acted upon for the first time; and he left the infirmary. Meanwhile he had suffered much from sickness and prostration, and had, moreover, been seized with hiccough.

The next appearance of the tumour was at noon on the day of his admission to the Belfast Hospital. Almost immediately afterwards he felt intensely sick, with a sensation of extreme prostration. He now sought further assistance, and a medical man applied a few leeches, and recommended him to go to hospital.

On the morning of the 27th, when I first saw the case, I found the right side of the scrotum distended by a tense globular tumour, as large as an orange, not tender on pressure, except at its neck. It could be traced up into the inguinal canal, which seemed slightly swollen. The testicle was undistinguishable; but the cord was to be felt running along the back of the swelling. Not the smallest impulse could be detected upon making the patient cough. The leeches applied before admission had caused free bleeding, which continued most of the night. He had not slept any in spite of a large opiate which had been given after admission. During the night, also, he had vomited a great deal; was suffering from constant nausea, and from great pain in the belly, which was tympanitic.

From the previous history and the present condition of the patient, although he quite repudiates the idea of having a rupture, I concluded that I had to deal with a case of strangulated congenital hernia, with probably a tense stricture demanding immediate relief. I decided that the best course would be to try the taxis, under chloroform, as soon as possible, and should moderate efforts then fail, to operate forthwith. As the case was by no means free from obscurity, I requested my colleagues to meet me in consultation, in one hour's time, and directed that the patient should, meanwhile, be kept in a warm bath.

After the interval just mentioned had elapsed, the patient, well wrapped up, was removed to the operating theatre. My colleague, Dr. Murney, the only one able to attend, agreed thoroughly with me in my view of this somewhat difficult case. Chloroform was administered, and the taxis was then tried. The stricture, however, seemed so tight, and the parts involved so tense and unyielding, that the attempt had speedily to be abandoned, and before the patient had recovered from the influence of the anesthetic I proceeded to perform the operation. The intestine had now been prolapsed for twenty-six hours, and might be considered to have been strangulated almost from the very first.

I made an incision some three inches in length in the usual situation, carefully divided several layers of fascia, and exposed the hernial sac. This was now opened at the inferior part of the wound, as there appeared to be no prospect of reducing the hernia without doing so. About an ounce of light-coloured serum escaped; and between seven and eight inches of much congested small intestine were exposed. There was no omentum. Passing the forefinger into the neck of the sack a constriction was detected at the external ring, and divided. I now tried gently to pull down the intestine previous to returning it. I found it was quite immovable, even when firm traction was resorted to. I then re-introduced my finger, and after passing it up the full length of the inguinal canal, which was dilated, and apparently unshortened, I reached with some difficulty, a second stricture at the internal ring. Through this, after considerable manipulation, I insinuated the tip of my finger, and found a stricture so tight that it felt just as if a piece of cord had been tied tightly around it. At such a depth from the surface, fully two inches and a-half, it was at once difficult and dangerous to attempt to divide the constricting ring. I did so, however, by passing a straight blunt pointed bistoury flatwise along the front of the finger, by which the edge was guarded, until the end had got beneath the ring. I then turned the edge directly upwards, and freed the constriction. I certainly could not have performed this part of the operation with either ease or security had I used any of the different forms of directors, or a curved knife, in place of a straight-bladed one.

The bowel was now easily drawn down, and the portion included in the stricture was, I observed, deeply sulcated. During the return of the intestine I saw the testicle lying exposed at the bottom of the scrotum, and contained in the hernial sac, thereby establishing the correctness of the diagnosis.

The evening of the operation I found my patient much easier. He was still inclined to vomit; and the abdomen, on examination, was found painful and tympanitic. Calomel and opium, and a fetid enema were ordered. Next day a new and very unusual symptom declared itself. The right testicle had become enlarged and exquisitely tender; and I subsequently ascertained that suppuration of the hernial sac had also taken place. On the seventh day after the operation I found it necessary to lay open the cavity of the tunica vaginalis, by extending downwards the original wound, and so give issue to four ounces of laudable pus. When this complication had been overcome the case proceeded satisfactorily enough.

The wound rapidly closed, and the testicle, which had been much enlarged, returned to its normal size. On the fourteenth day after the operation the patient was able to sit up, and was pronounced convalescent. Soon afterwards he left the hospital with his general health in every way improved. The local disease was cured; for, in consequence of the inflammatory action set up in the parts the inguinal canal had become blocked up or obliterated. There was no tendency to protrusion when he was discharged; and some months afterwards I had an opportunity of ascertaining that he did not require to wear a truss. A radical cure, in short, had been effected.

“Nothing,” says Sir Astley Cooper, “but great want of attention can cause a hernia to be mistaken for an enlargement of the testis.” Yet in this case that mistake, I believe, had occurred in three different instances. The patient’s own statement that he had no rupture seems to have been adopted without dispute, and he was treated by purgatives and leeching, just as if the disease had been orchitis. The diagnosis of the case was certainly not altogether free from difficulty; for, in addition to the patient’s own assertions that he never had had rupture, the absence of all impulse on coughing, and the comparatively small diameter of the neck of the sac involved some doubt. On the other hand, the history and the symptoms clearly showed the case to be one of hernia proper, and not hernia humoralis, or hydrocele.

This, like the majority of congenital herniæ, required the opening of the sac; and the circumstances here detailed show the necessity of the procedure. The contents of the sac were those most frequently found—omentum being rarely present in hernia into the vaginal process of peritoneum.

The importance of recognizing this form of rupture in the adult is very great. It was pointed out by Dupuytren, and subsequently by other observers, how frequently congenital herniæ were strictured at the internal ring, that the course run by such cases was very acute, and that, in consequence, relief must be promptly afforded. They occur, indeed, more frequently than is commonly supposed; and, as in the two cases here described, are generally found upon the right side. In the majority of instances some portion of the vaginal process remains open for some time after birth; and it is found longer unclosed upon the right than upon the left side.

My third case was that of a man sixty-four years of age, one who had been a domestic servant, then for many years a soldier, and who is now a day labourer. He was admitted into hospital at

2 p.m., on the 31st January, 1866, with a strangulated inguinal hernia on the left side. He stated that he had been subject to rupture for thirty years. It first appeared as a small tumour in the groin, but afterwards gradually formed a large swelling in the scrotum. For many years he kept the protrusion up by means of a truss. This had recently been broken, and the imperfect contrivance of his own with which he replaced it, permitted the frequent descent of the bowel. On the day before his admission to hospital, at 10 a.m., the swelling formed in the scrotum, and he found himself unable to return it as usual. Very soon he began to vomit, and to suffer great pain both in the part itself and over the abdomen. Two medical men who saw him tried the taxis four times that evening, and once the next morning, without success. The symptoms increasing in severity, they recommended his admission into hospital. When I saw him, shortly afterwards, I found the left side of the scrotum distended by a tumour as large as a cocoa nut, measuring, indeed, nearly eight inches in length, by three and a-half inches in diameter. The neck of the tumour was thick, and the lower part of the inguinal canal was much distended. The spermatic cord could be traced running up the posterior surface of the protrusion, and the testicle indistinctly felt at its lower part. The swelling had the unusual hour-glass form, caused by a well marked constriction just below the external ring. Above this the inguinal canal was partially dilated by a globular tumour, much smaller than the tumour below the partial septum. Scarcely any impulse was communicated to the part when the patient was told to cough. He felt very sick, and complained of severe pain in the left inguinal region, which was extremely tender when pressure was made over the neck of the sac.

I directed the patient to receive a full dose of opium, and to be placed in a warm bath. While he was in the bath I tried the taxis for a short time without effect. The intestine had now been strangulated for twenty-six hours; and in order that no further time might be lost the patient was brought under the influence of chloroform about two hours after his admission. Very gentle efforts were made to reduce the tumour, but without effect, and my colleagues, then present, agreeing with me that relief should be given, the operation was at once commenced.

An incision, commencing over the internal ring, was made for four inches downwards towards the scrotum; the subjacent layers of fascia were carefully divided, and the sac was exposed; across it

ran a tense fibrous band corresponding to the hour-glass-contraction; this band was incorporated with the tissue of the sac, which was necessarily opened when dividing it; a little dark-red serum escaped, and some omentum protruded from the wound. I now tried to effect reduction, but failed; and on introducing the finger a little way easily discovered a second stricture at the internal ring, which was cautiously divided; the edge of the knife being merely pressed against the tense fibres of the ring, which readily gave way before it; the forefinger of the left hand was here also used as a director, which gives both confidence to the operator, and directs with certainty the motions of his knife; the omentum was now returned, and then the small intestine which came into view; the first portion of the intestine was but little congested, but the latter portion, which lay in the inferior division of the sac, and had been acted upon by both strictures, was purple-red; upwards of three feet of the small intestine were replaced, inch by inch, in the abdomen before the sac was emptied; five points of interrupted suture were inserted in the wound, and the usual dressings were applied.

During the evening and night of the day after the operation the bowels acted three times very freely, and this in spite of the opium taken to prevent it; and throughout the entire convalescence the tendency to diarrhea could only partially be kept in check; no other complication occurred during the after-treatment, save that the left testicle became slightly enlarged; the wound slowly healed, and was all but closed upon the twentieth day after the operation; the patient was then able to sit up, and is reported as being quite convalescent. He was discharged from hospital a few days afterwards, and recommended to wear a light truss.

This case, whose history is distinctly that of the ordinary inguino-scrotal variety of hernia, is unusual on account of the hour-glass-contraction at the upper part of the sac. This peculiarity, Mr. Birkett alleges, is always associated with a congenitally open condition of the vaginal process of peritoneum. The great length of intestine, contained in this hernia, added very much to the danger attendant upon an operation; besides, the repeated trials of the taxis, and the diarrhea which subsequently manifested itself, were most unfavourable features in the case. “*Les selles promptes apres l’operation,*” says Vidal, “*sont d’un mauvais augure.*” Notwithstanding this—and the man’s previous habits, which had been much the reverse of temperate—the convalescence was not tedious.

The fourth case was that of an old man, eighty years of age;

he had been employed nearly his entire life either in whiskey-making or in whiskey-selling; he was for many years employed in the Belfast Distillery, and for upwards of thirty had kept a public house; when a young man he used to drink enormous quantities of whiskey—often as much as a quart, he said, fresh from the still, where the spirit is about sixty-five degrees over proof; he did not, however, consider himself a “great drinker;” until his present illness he had never, he said, been in the doctor’s hands. He was admitted to hospital, with strangulated inguinal hernia of the left side, on April 7th, 1866, at half-past eleven at night. About four years ago he discovered that he laboured under rupture of the right side, and about two years later he found that a rupture had taken place in the left groin, the one that had become strangulated. The history he gave of himself was that, while in bed, between four and five, a.m., on the day of his admission, the gut on the left side protruded, and he was unable to return it; within half an hour sickness and vomiting, with severe abdominal pains, set in, followed soon after by hiccough; all day he expected the tumour would go up again, and took, for the purpose of aiding its return, senna and salts, and two purgative pills, which, fortunately, he threw off; as evening came on the pain grew more severe, and the prostration, sickness, and vomiting became worse, while the stomach ceased even to retain cold water. Dr. Harkin, who was called in at ten p.m., now requested me to visit him; and, as I found there was neither comfort or convenience where the poor old man lived, I merely ascertained the nature of the case, and had him removed to hospital; on examining him there I found a small reducible hernia in the right inguinal region, while in the left there was a tumour in the groin, situated above Poupart’s ligament—very tense, hard, and sensitive; the abdomen was tympanitic, and the pulse very weak; he complained of great pain in the *scrobiculus cordis*; was much depressed, and believed himself about to die; nothing was retained upon the stomach, and the constant hiccough was very distressing. His case, considering his advanced age, the urgent nature of the symptoms, and the rapidity with which they supervened upon the descent of the hernia, was not a very hopeful one. The tumour more resembled a tightly strangulated femoral hernia than an inguinal one, which, however, it distinctly was. The man’s condition admitted of no delay; he was at once placed in a warm bath, where, for a short time, I vainly tried the taxis; in the operating theatre, to which he was soon afterwards removed, I made

another attempt to reduce the hernia while the patient was under the influence of chloroform; this trial merely confirmed my previous impression that a very tight stricture, which the taxis was not likely to overcome, existed; accordingly I determined at once to perform herniotomy. An incision through the skin and superficial fascia was first made commencing over the internal ring, and extending for three inches downwards towards the scrotum in the axis of the inguinal canal; three different layers of fascia were divided, and a thickened sac, with some bands constricting it in the region of the external ring, was exposed; these bands were divided in the hopes of returning the protrusion without opening the sac, but as the stricture was evidently seated elsewhere, it was necessary to open the sac, which I did at the lowest part of the wound; and in doing so, so tense was it, I experienced the greatest difficulty in pinching it up between the blades of the forceps previous to incising it; about an ounce and a half of reddish serum escaped, and a single knuckle of small intestine was found to be the only protruded portion of the abdominal contents; the gut was greatly congested, and so sharply folded upon itself that one portion of its concave border, about the size of a sixpenny piece, was chocolate-brown in colour, and seemingly in a state not far removed from becoming gangrenous. On introducing the finger a very tight stricture was felt, at the internal ring; I freed this to, what I considered, a sufficient extent, with the knife guarded, as before, but could not return the protrusion; I, therefore, reintroduced the bistoury, and made a second incision in the stricture, being afraid of the results which might follow any rough or protracted handling of the bowel; still, notwithstanding the second incision, I found myself unable to reduce the hernia; and it was not, strange to say, till I had a third time cut through the tense fibres of the ring that I was able, without using violence, to replace the intestine within the abdomen; four sutures were now introduced, and water dressings, and a bandage were applied; the patient, who was in an extremely low state from the combined effects of the disease and the operation, was cautiously removed to bed, and a glass of brandy and water, with thirty minims of laudanum in it, was administered to him.

On seeing the patient next morning, about nine hours after the operation, he told me he felt himself "quite a new man." The pain and sickness, and other symptoms, had almost completely subsided; he was ordered nutritious diet, and a glass of whiskey daily. On the fourth day the report states that he had slept the entire of the

previous night, and that the wound had almost completely healed by the first intention, save at the centre, from which there was a small oozing of pus. On the eighth day the bowels were open—naturally; and on the twelfth day after the operation the report is:—"The wound is completely healed, the bowels are acting daily, and the patient's general health is greatly improved." In short, he was perfectly convalescent; and left hospital, wearing a light truss, soon after.

The case just related is, in several respects, remarkable, especially when we consider the patient's very advanced age. Other surgeons may have had occasion to operate on persons as old, or even older; but similar occurrences must be rare. Mr. South, of St. Thomas' Hospital, had a case under his care in which a successful result followed an operation for inguinal hernia in a man eighty-two years of age. Independently of my patient being ten years beyond the prescribed limit of life, his previous habits had not been of a nature much to conduce to a satisfactory termination of so grave an operation; he had, in fact, been very intemperate; and, latterly, lived in extreme poverty. It is very unusual to find in one whose tissues must necessarily be relaxed by great age a hernia become strangulated with such great rapidity and severity; and it justified the inference, afterwards borne out during the operation, that the stricture was a tight one, as it proved necessary to use the knife to free it, three different times, before the rupture could be reduced. To the circumstance that no time was lost in giving relief after assistance had been called in, and to the fact that the taxis was very sparingly employed, I ascribe, in a great measure, the very successful issue. No efforts at taxis, indeed, however well directed, would have forced the intestine through the stricture; and such efforts would, in all probability, have resulted in irreparable damage to the highly-congested gut. The operation was performed about an hour after the patient's admission to hospital, and about twenty hours after the first descent of the bowel. Three-fourths of the wound, which, as before stated, had healed completely on the twelfth day, were as perfect an example of immediate union as could be desired. This is a rare mode of union under any circumstances, but especially so taking into account the age, and other antecedents of the patient.

I would only say, in conclusion, that in none of these four cases was the opening of the hernial sac attended or followed by any of those serious consequences which some surgeons think there is so much reason to dread.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Clinical Notes on Uterine Surgery, with Special Reference to the Management of the Sterile Condition. By J. MARION SIMS, A.B., M.D.; late Surgeon to the Woman's Hospital, New York, &c., &c. London: Hardwicke.

THE author of this book describes it as a collection of clinical notes, laying no claim whatever to the character of a systematic work. "It is simply a voice from the Woman's Hospital which in all probability would never have been heard if I had remained at home." If this be so we must congratulate ourselves that the political troubles through which America has recently passed drove Dr. Marion Sims to Europe, inasmuch as it has led to the publication of this most instructive and valuable work, one remarkable alike for the vigour and clearness of its style, the ingenious adaptation of treatment to morbid conditions, and the earnestness and single-mindedness it evinces as the characteristics of the author.

Dr. Sims did not propose to himself to write a complete monograph on uterine surgery or on the treatment of sterility, but simply to interweave the two, while taking a glance at such surgical difficulties as seem ordinarily to interfere with conception; but we must say he has in so doing given us a work replete with information on almost all the forms of disease usually met with in the female organs of generation. He arranges his cases into two classes. 1st. Natural sterility—those who had never conceived; and 2nd. Acquired sterility—those who had, but for some reason had ceased to do so for a time, say five years or more; and on looking over his notes for a series of years he was surprised to see how nearly equal these two classes were; that is taking all cases of uterine disease as they are promiscuously presented, though of those who seek advice merely for sterility, without reference to disease or actual suffering, the first class will, of course, greatly predominate. It is only by the study of all, he says, that the conditions can be

ascertained that are most opposed to conception, and, at the same time, those most favourable to it. In the following passage we have a summary of the unfavourable conditions:—

“The trouble in one case may depend upon mere contraction of the os: in another, upon malformation of the same—in another upon engorgement of the cervix—in another upon elongation—in another upon hypertrophy—in another upon simple induration—in another upon curvature of the canal of the cervix—in another upon polypus—in another upon a fibroid—in another upon malposition of the uterus—in another upon some anatomical anomaly or malformation of the vagina—in another upon vitiated secretions of the cervix—in another upon those of the vagina, the one generally acting mechanically, the other chemically—in another upon the absence of spermatozoa; while others may be complicated with several of these anomalies, all subjects of study and investigation.

“And when we come to analyze these various causes and complications, they are resolved into one great general principle, embracing all manner of obstructions to the free passage of living spermatozoa into the cavity of the womb. In all curable cases ovulation must be perfect, and the faulty link must be found in defective fructification, or else all our efforts are in vain. If the woman has passed the period of ovulation, of course we can do nothing for her. If the ovum never passes into the fallopian tubes, a thing impossible to determine, it is equally beyond remedy. We may safely assume a normal menstruation as a sign of normal ovulation. This being our guide, we may hope, in the majority of cases, to find some of the troubles above enumerated, many of which are eventually curable.”

The conditions essential to conception are stated as follows; and Dr. Sims takes them up *seriatim*, and proposes to show, from clinical experience, wherein the sterile condition differs from the fecund, and to point out, so far as is known, the direct methods of relief:—

“1.—It occurs only during menstrual life.

“2.—Menstruation should be such as to show a healthy state of the uterine cavity.

“3.—The os and cervix uteri should be sufficiently open to permit the free exit of the menstrual flow, and also to admit the ingress of the spermatozoa.

“4.—The cervix should be of proper form, shape, size, and density.

“5.—The uterus should be in a normal position, i.e., neither anteverted, nor retroverted to any great degree.

“6.—The vagina should be capable of receiving and of retaining the spermatic fluid.

“7. Semen, with living spermatozoa, should be deposited in the vagina at the proper time.

“8.—The secretions of the cervix and vagina should not poison or kill the spermatozoa.”

Before entering more fully into the subject Dr. Sims devotes some consideration to the method of making a uterine examination, and describes his own plan. For examination by touch he prefers the dorsal decubitus, whilst for examination by sight he uses the left lateral; and in all cases the speculum known by his own name, or as the “duck-bill.” The dorsal decubitus is in many cases essential, especially if we wish to trace the outline of the uterus through the abdominal walls, either by the bi-manual method, so clearly explained by Dr. Sims, or by the use of the uterine sound; but it is more annoying to the patient, and not so advantageous, in our opinion, in the majority of cases, as the lateral, or ordinary obstetric position of this country. With regard to the speculum we would make a similar remark—in very many cases Sims’ speculum is useful beyond all others; and it presents this advantage, that with it we can touch with the finger, at the same time that we see clearly, the parts under examination; and we know of no other speculum by which this can be done; but in most cases Ferguson’s glass speculum answers every purpose, and is more easily used, and less likely to pain or annoy the patient. However, the usefulness of a speculum does not depend so much on its form as on the skill with which it is used; and whether it be Sims’, or Ferguson’s or Ricord’s is not so important as that it should be used with gentleness and care. The same may be said of the uterine sound, of which Dr. Sims proposes some modifications. We ourselves make constant use of Simpson’s original form, and see no reason to change it; we may sometimes wish it were made of copper or some substance more easily bent, but the marks for measuring the length to which it may have passed into the uterus, we regard as indispensable. Dr. Sims objects to them as making the instrument difficult to clean, but we certainly have never had any difficulty with it in this respect. On the whole we think Dr. Sims does not value the sound so much as we do. To one so expert its use may not be very essential, but to us it is the most valuable instrument in the *armamentarium chirurgicum*, enabling us, as it does, to touch, feel, and manipulate parts otherwise inaccessible. However these are matters of too little importance to keep us from the substance of the book.

The first section is devoted to the illustration of the proposition,

that "conception occurs only during menstrual life," we shall not dwell on this, but pass to the next, which shows that "menstruation should be such as to show a healthy condition of the uterine cavity." This section opens with the following definition of healthy menstruation, and of some of its deviations:—

"If asked what constitutes normal menstruation, I should reply, a painless uncoagulated flow, returning at intervals of about four weeks, lasting three, four, five, or six days, and requiring the use of not more than three, or, at the farthest, four napkins in the twenty-four hours. It may vary from a healthy standard in both quantity and quality. It may be scanty or profuse, and painful or not, without regard to quantity. If the flow falls short of three days' duration, it may be called scanty. If it continues longer than six or seven days, it may be profuse, but not always so. It may be very abundant, and last but two or three days; and, again, it may continue twelve or fifteen days, and be very scanty, requiring not more than one napkin in the twenty-four hours. The explanation of either of these conditions will generally be found in some organic deviation from a normal state.

"Conception may take place whether the menstruation be scanty or profuse. But either extreme is not very favourable to it, not that the amount of blood lost is *per se* an important matter, except as the index of an organic condition, favourable or otherwise to the fulfilment of this great law of nature."

Of scanty menstruation Dr. Sims has not much to say, but his remarks on the profuse form are of very great value. He does not think it often depends on constitutional causes, or can be cured by constitutional treatment. He knows very well, he says, it may arise from mere debility, from superlactation, and from some temporary engorgement of the portal system; but such cases are not very common, and not usually obstinate. He would never, therefore, put off a uterine exploration in any confirmed case of abnormal flow, for where there is an inveterate menorrhagia there will always, he says, be some organic cause for it:—

"It may be due simply to granular erosion; to engorgement of the cervix; to fungoid granulations in the cervical canal, or in the uterine cavity; to polypi of the os, the cervix, or the cavity; to a fibroid tumour, intra-uterine or intra-mural; to inversion of the uterus, to hematocele; or it may be a sign of some malignant degeneration, all giving rise to hemorrhage, and each requiring its own peculiar and appropriate management.

“I propose to illustrate, from clinical experience, the surgical treatment of menorrhagia as it may originate from one or the other of these sources.”

Cases of menorrhagia from each of these causes are now given, each of which may be regarded as a type, and is made the starting point for remarks on the treatment of the class to which it belongs. For the treatment of granular erosions Dr. Sims makes use of chromic acid. A drachm of this salt is dissolved in a drachm of distilled water, and is applied to the granulations by means of a solid glass rod, which is only allowed to take up a drop or two. It produces no pain, and may be carried into the canal of the cervix, or even further.

The case of menorrhagia, from fibrous engorgements of the cervix, was one of acquired sterility; the neck of the womb was the seat of fibrous engorgement, with superficial granular erosion. It was considerably hypertrophied and indurated. The organ was in its normal position. The thickened and indurated lips of the os uteri were, in consequence of their hypertrophy, in close apposition, the one against the other; thus mechanically closing the os, although it was large enough to admit a No. 8 bougie. The granulations were cured by the use of chromic acid; still the menorrhagia continued; the cavity of the uterus was explored by means of a sponge tent, but there was nothing abnormal there. It was now determined to incise the os, for which there were two reasons:—1st. Division would probably ameliorate the engorgement and lessen the hemorrhage. 2nd. It would separate the lips so as to allow of the entrance of spermatozoa, and permit conception, which it was hoped would cause a complete revolution in the nutritive functions of the whole organ, and ultimately a perfect cure. In three months after the operation menstruation was perfectly natural, and in nine months more the patient was delivered of a fine vigorous child, after an acquired sterility of nine years.

Fungoid granulations, resembling those so often seen in burns and chronic ulcers, are sometimes the source of menorrhagia. They may be found in the canal of the cervix, or in the cavity of the uterus, or in both at the same time; so that when a vaginal examination fails to ascertain the cause of the profuse flow it is necessary to explore the cavity of the cervix and of the uterus itself. This is to be done by the use of sponge tents. For the manufacture and use of these very full directions are given, differing but little

from those originally given by Sir James Simpson; but Dr. Sims has ascertained the very interesting and important fact that the use of these tents will not merely dilate the uterus, but by their pressure cause the removal of granulations, and even of mucous, and, in some cases, fibrous polypi. Their use, however, is not devoid of danger. At first Dr. Sims allowed his patients to go about with them in, and allowed them to be retained in the uterus twenty-four hours, or more; but he soon found that metro-peritonitis or other serious mischief might be produced in this way, and he became convinced that they should not be applied unless the patient consent to remain indoors, if not in her bed-room, for a week; and this even when only used for a day; and he now leaves the tents in the uterus little more than five or six hours. In this way he believes the use of tents as safe as any other remedy capable of doing good; and he finds that placing a piece of cotton, saturated with glycerine, in the vagina prevents their acquiring the peculiarly offensive smell that is so often an objection to their use.

Menorrhagia from polypus is the next form considered; viewing polypi surgically Dr. Sims classifies them topographically as follows:—

1st. Those growing from the os tincæ.

2nd. Those growing in the canal of the cervix.

3rd. Those growing in the cavity of the uterus.

Referring to the hemorrhage that sometimes follows the removal of polypi by the scissors, Dr. Sims narrates his experience of the use of the styptic salts of iron, of which he prefers the persulphate, as he has found it difficult to obtain the perchloride free from acid, which, when present, irritates the mucous surface of the vagina. In our own practice we make constant use of the saturated solution of the perchloride in glycerine, as recommended by Sir James Simpson, and have had no reason to complain of its causing any irritation. After using the solution of iron Dr. Sims applies cotton saturated with glycerine, a dressing that he employs in all his surgical operations on the neck of the womb, and in other cases of organic lesion. From the great affinity of glycerine for water he believes it causes a capillary drainage by osmosis, depleting the tissues with which it comes in contact, and giving them a dry, clean, and healthy appearance.

In the diagnosis of polypous tumours Dr. Sims believes his method of exploration cuts the Gordian knot; and he relates a case that had been regarded as one of carcinoma, till examination with his

speculum showed it was a mushroom-shaped fibrous polypus, growing from the os, and so completely covering and forming a cap for it, that to the finger it presented only a knobby hard growth, occupying the place of the cervix, and the os could not be felt.

Small polypi in the canal may be removed by the pressure of a sponge, cut off with scissors, or pulled off with a forceps; for this latter purpose Dr. Sims recommends M'Clintock's fenestrated forceps, of which he gives a figure. For large polypi, where the scissors cannot be used, Dr. Sims recommends the *écraseur*. He has added a *porte-chain* to Chassaignac's instrument, by which the chain may be rendered stiff, and carried into the vagina or the cavity of the uterus as easily as a sound, and then expanded so as to facilitate its being passed over the tumour. Dr. Sims gives cases illustrating the several varieties of polypi, and closes the series with the following:—

“Time was when women died of polypi without any effort being made for their relief. This is not so now. No delicate operation is easier; none more successful. Life is sometimes lost because we think the patient so near death that any interference would only accelerate the fatal issue. This is a great mistake. To save life where death is imminent, we are justified in assuming great responsibilities and even of taking great risks. I fear that we sometimes hesitate to do our duty by asking ourselves the question, ‘How will it affect me if I fail?’ It has been said of a great American lithotomist that he often refused his skill to bad cases because they might spoil the statistics of his unparalleled success.

“In December, 1861, Mr. Preterre, an eminent American dentist in Paris, asked me to see Madame R., in consultation with her physician. She had menorrhagia for many years, and was extremely prostrated by it, and by a profuse muco-purulent vaginal discharge, which had been present for six or eight months whenever the hæmorrhage ceased. She had been seen by many of the most eminent surgeons in Paris, but no one suggested anything for her relief. I found the uterus retroverted and greatly enlarged, the fundus extending quite to the hollow of the sacrum, and seemingly filling up the whole of this region. A glance showed at once that it could be but one of two things—a polypus or a fibroid tumour. The os *tincæ* admitted the end of the index finger. I was anxious to determine the nature of the case, and made gentle but persistent pressure for some minutes through the cervix. It gradually yielded to the force, and the finger, gliding into the cavity of the uterus, detected an enormous fibrous polypus, which could not pass outwards because of the retroflexion. I was obliged to be in London the next

morning, but I promised to return to Paris in a week, for no other purpose than to apply a sponge tent and remove the polypus for Madame R. Five or six days after my departure they telegraphed to me that she was much worse; that a consultation of physicians had decided that it was now too late to attempt any operation, and therefore that it was unnecessary for me to return to Paris. Fortunately, the telegram was not received, and I returned to Paris to find my patient in a state of complete exhaustion. She had a profuse, dirty, offensive, sero-sanguinolent discharge from the vagina, which poisoned the atmosphere of her apartment. Her pulse was small and rapid; she was quite anæmic, and presented all the appearances of blood-poisoning. On passing my finger into the vagina, I found it entirely filled by an immense fibroid polypus in a state of decomposition. She was evidently dying from the absorption of the detritus of this fetid mass. At my first visit, a week before, this tumour was wholly intra-uterine, but now it filled the vagina. I infer that its escape from the cavity of the uterus was due to powerful contractions provoked by the forcible introduction of the finger for exploration, for she grew worse from the moment of my visit. She had forcing pains, as of labour, for a while, and afterwards passed into the low condition in which I found her. Its pedicle (as is most usual) grew from the anterior wall. What was to be done? There was assuredly but one course to pursue. If we allowed this great mass to remain there and slough away, death was absolutely certain. Its speedy removal gave the only hope of rescue. Her physicians consented to its écrasement, which occupied ten or twelve minutes. Vaginal washes, wine, and generous diet soon completed the cure. If I had received the telegram, she would certainly have died, and I should have been censured by her friends for hastening the fatal issue, inasmuch as my previous visit was the inauguration of a new phase of her sufferings. If I had been afraid to operate because she was almost in a moribund state, she would unquestionably have been lost.

“I have related this case perhaps too minutely, but it is to encourage the young man never to falter in the clear path of duty to his patient, and to show that extreme exhaustion is no barrier to the mere operation; for, when effected by the écraseur, there is no danger of hemorrhage, and very little of any other character.”

Menorrhagia from fibrous tumours and that from inversion of the uterus are next considered, and then Dr. Sims passes on to painful menstruation. As a general rule, he does not think fibrous tumours ought to be interfered with unless they endanger life; and then he prefers Dr. Savage's plan of injecting tincture of iodine into the uterus—having first dilated the os with sponge—to the other plans of treatment.

Painful menstruation, Dr. Sims believes, to always depend on mechanical obstruction. "I lay it down as an axiom," he says, "that there can be no dysmenorrhea, properly speaking, if the canal of the neck of the womb be straight, and large enough to permit the free passage of the menstrual blood." At one time Dr. Sims believed menstruation might be painful without any mischief or impediment, simply because Dr. Bennet and others said so; but now he is convinced that there is no such thing as "constitutional dysmenorrhea." "Painful menstruation is only a symptom of disease, which may be inflammation of the cervical mucous membrane, retroflexion, antelexion, fibroid tumour in one wall of the uterus or the other, contraction of the os internum or externum, flexures of the canal of the cervix, either acute or gently curved, either at the os internum, at the insertion of the vagina, or extending throughout the whole length of the canal; all of which are so many mechanical causes of obstruction which must be recognized and remedied if we expect to cure dysmenorrhea."

This same view has recently been so fully illustrated in a paper on dysmenorrhea, read by Dr. Kidd before the Obstetrical Society, and published in our number for November last, along with an account of the methods of treatment founded on it, that it seems unnecessary to dwell on the subject at present.

III. The third section of the work illustrates the third of the propositions laid down at the commencement, viz.:—"The os and cervix uteri should be sufficiently open not only to permit the free exit of the menstrual flow, but also to admit the ingress of the spermatozoa." Cases are given proving that though there may not be dysmenorrhea or other symptoms of disease, the os may be too small, or so placed as to prevent the entrance of spermatozoa. This condition may either be natural or acquired, the os being often closed as the result of the injudicious use of caustic, and for its removal the os must be incised as in dysmenorrhea. Under this head Dr. Sims figures a condition of the os that we have frequently met with, where a projection on the anterior lip gives the os a crescentic shape and forms a valvular obstruction to the entrance of spermatozoa. A case of this kind is detailed where the projection was excised, and conception soon followed; and other cases are given where the posterior lip was the cause of the obstruction from its being so long as to overlap the opening; in some of which it was removed, and in others slit antero-posteriorly, with happy results.

IV. "The cervix uteri should be of proper size, form and density."

Two deviations from the natural size are spoken of here:—1st. Elongation; 2nd. Defective development. For elongation amputation is recommended, and the method of doing it is described; and for some cases of deficient development of the cervix slitting the os is suggested, but no case is given in which this had been done.

In form the cervix may vary from the normal by being conical, long, or curved; and here also amputation is recommended, and a guillotine-shaped instrument is recommended for the purpose.

For induration of the cervix, the remaining subject treated of in this section, Dr. Sims has nothing new to suggest. The use of caustic potash he does not recommend; he has found Dr. Churchill's iodine treatment produce a greater amelioration than anything else, but it is tedious, and often fails; where there is much fibrous deposit Dr. Sims incises the os; and where it is possible to amputate the indurated part, he does so.

V. "The uterus should be in a normal position, *i.e.*, neither anteverted nor retroverted to any great degree."

In the following table Dr. Sims shows how frequently he has met with these forms of malposition in his two classes of natural and acquired sterility:—

	"No. of Cases	Anteversions	Retroversions	Total Malpositions
1st Class ...	250	103	68	171
2nd Class ...	255	61	111	172
Total ...	505	164	179	343

"Thus we see in 250 married women, who had never borne children, that 103 had anteversion, and 68 retroversion; while in 255 who had once borne children, but for some reason ceased to conceive before the natural termination of the child-bearing period, 61 had anteversion, and 111 retroversion, the sum total in each class bearing almost exactly the same relation to the number observed, being about two-thirds of the whole. Hence we infer that if the malposition exercises an influence to prevent conception in the one class, it is of equal importance in preventing it in the other. The mere position of the uterus is here stated without reference to causes or complications. I have purposely avoided saying how many of these had granulations, engorgements, hypertrophies, fibroids, ovarian cysts, or other complications. The table shows that two-thirds of all sterile women labour under some form of uterine displacement, without reference to the particular cause of such displacement;

and that the anteversions and retroversions in the two classes are in inverse proportion: the anteversions in the first being about equal to the retroversions in the second; and the retroversions of the first nearly the same as the anteversions of the second."

The pathology and mechanism of these conditions are very fully considered in this section, but Dr. Sims is not very hopeful as to the treatment of anteversion:—

"So far as the treatment of the sterile condition in connexion with anteversion is concerned, I fear that our efforts must be confined almost wholly to seeing that the os tincæ is open enough, that the cervix is of proper form and size, and that the secretions of the vagina and of the cervix are suited to the viability of the spermatozoa."

In some aggravated cases Dr. Sims has corrected the anteversion by shortening the anterior wall of the vagina; but, as a rule, the only treatment he recommends is a ball pessary, or rather a ball of cotton and glycerine, placed in the anterior *cul de sac* of the vagina.

Dr. Sims objects very much to the use of the sound for correcting displacements. He scouts the idea of curing them by even daily replacing the organ in its natural position. The sound may be used to correct retroversion, but for this purpose it is, in his opinion, a most dangerous instrument, and for anteversion it is utterly unnecessary. For retroversion he suggests a method of manipulation by a species of double touch, one hand acting from the surface of the abdomen, one from the vagina by means of a sponge probang. He has also devised a jointed sound to be used as a "redresser;" but as we are in the habit of using Simpson's sound for the purpose almost daily, and have never found any injury to arise from its use, and as the multiplication of instruments is an evil, we do not intend at present to provide ourselves with Dr. Sims' redresser. For retaining the uterus in its place after the retroversion has been corrected Dr. Sims is very warm in his commendations of the closed lever pessary, invented by his countryman, Dr. Hodge, the same that we have so frequently recommended to our readers in the pages of this Journal. In some rare cases, where the vagina is too irritable to bear any instrument, it is necessary to use cotton, and Dr. Sims figures an instrument he has devised to enable patients to introduce these themselves.

Though not included in the heading of the chapter, procidentia uteri is also discussed in this section. In addition to the ordinary forms of procidentia, where the uterus is elongated, and the os forms

the lowest part of the tumour, the fundus still remaining within the pelvis, Dr. Sims says he has met a few cases where the whole uterus has descended through the vulva, and describes one shown to him by Dr. Chepmell, of Paris, in a maiden lady some forty years old, who had suffered from it twelve or fifteen years. The great peculiarity of this case consisted in the fact, that the uterus was but one inch and a half deep. This is a rare form. At present we have a case of it under observation in a woman between fifty and sixty years of age, who has borne children. Prolapse of the posterior wall of the vagina seems to have occurred after the birth of her first child; then, on making some exertion, under the influence of a strong mental emotion, she felt something slip through the vulva, which seems to have been the uterus, and has remained down ever since, except when pregnant. She has had one child since, at the full period, and a miscarriage.

The tumour lies outside the vulva; the os uteri is directed forwards; the fundus is near the anus; the uterine cavity measures the normal length. In the descent of the tumour the posterior wall of the vagina first protrudes, till the os makes its appearance at the vulva, and then the fundus slips into the pouch formed by the prolapsed posterior wall of the vagina. The anterior wall of the vagina does not come down at all. The more common form of prolapse is associated with longitudinal hypertrophy of the uterus—a condition first fully described by Huguier. In this form the utero-cervical canal is often found four or five inches deep; the supra-vaginal portion of the cervix being slender and attenuated; and, when examined per rectum, feeling not larger than the finger. Dr. Sims says this is evidently secondary—a statement that quite accords with our own experience; and, on several occasions we have demonstrated to the hospital class, that when the uterus has been properly supported by a pessary the elongation disappears. Within the last few days a patient presented herself for whom a pessary had been applied about twelve months before; at that time the uterus measured five and a half inches in length; the pessary had kept it well in its place; and, on examination, it measured not more than three inches.

It was necessary to remove the pessary for a time; and, at the expiration of a week, the os was again found to protrude through the vulva, and the cervico-uterine canal to be again upwards of five inches long. Our own experience shows us that in the great majority of cases a well-fitting pessary will correct this displacement,

and, for this purpose, we find the boxwood disc pessary the most useful. In using it, however, Dr. M'Clintock's caution as to the size of the central aperture, must always be borne in mind, as, if too large, the cervix will pass through and become strangulated. In young women, however, anxious to have children, and in some aggravated cases, operative means may be necessary; but, where this is not required, the parts may be supported by the pessary we speak of, or by cotton, as suggested by Dr. Sims.

Three methods of operating are now before the profession:—

1st. Amputation of the cervix, as suggested by Huguier, and which we quite agree with Dr. Sims in restricting to those cases where the elongation affects only the portion of the neck that lies below the insertion of the vagina—a condition for which we have, ourselves, operated with complete success.

2nd. The perineal operation, as performed by Baker-Brown, Savage, and others. To this Dr. Sims objects that the new perineum made by it often gives way in consequence of the pressure of the parts above.

3rd. The operation of narrowing the vagina by the “trowel or triangular shaped” denudation on its anterior wall, as performed by Dr. Sims and Dr. Emmet.

This latter is rather a complicated operation, but Dr. Sims speaks very favourably of it; he has never known it fail but once, and the patient was subsequently cured by a repetition of it in a slightly modified form.

VI. “The vagina must be capable of receiving and of retaining the spermatic fluid.”

In this section Dr. Sims proposes to review, first, the usual obstacles to the introduction of the semen, and then the conditions that prevent its retention or sojourn in the vagina. The ordinary obstacles to its introduction are mostly anatomical or mechanical, and may be arranged as follows:—

1st. The hymen may be imperforate, or nearly so.

2nd. There may be vaginismus, *i. e.*, hymeneal hyperesthesia, with a spasmodic contraction of the sphincter vaginae.

3rd. There may be atresia of the vagina.

4th. The vagina may be wanting.

Of these the most important is vaginismus, for the treatment of which Dr. Sims recommends the removal of the hymen, and division of the sphincter, with subsequent dilatation.

The circumstances that prevent the retention or sojourn of the

semen in the vagina Dr. Sims believes to be shortness of the vagina, and retroversion of the uterus.

VII. "For conception, semen with living spermatozoa should be deposited in the vagina at the proper time."

To some passages in this and the preceding section some critics have made grave objections. We must refer our readers to the chapters themselves, for a mere abstract of them would only lead to misapprehensions; but we must beg of them to remember, in reading them, that "to the pure in mind all things are pure."

VIII. "The secretions of the cervix and vagina should not poison or kill the spermatozoa."

The vagina and canal of the cervix each secrete a mucus peculiar to itself—that of the vagina is acid, that of the cervix is very slightly alkaline; and both may be altered by disease, so as to kill the spermatozoa. For vaginitis, Dr. Sims speaks highly of cotton saturated with glycerine, pure or containing astringents in solution; he also speaks favourably of medicated pessaries, both for vaginal and cervical leucorrhea; for the latter he has them made so that they can be passed into the canal of the cervix. Endo-metritis is another source of unhealthy discharges; and with some remarks on this the last chapter ends.

"A book, sir," said Dr. Johnson, "cannot contain more than the full of it;" but we assert that this book not only contains the full of it—and that of right good quality—but, by its suggestiveness, is worth even more than that which it contains.

Collection de Memoires sur une Fonction peu Connue du Pancréas, la Digestion des Aliments Azotés. Par LUCIEN CORVISART, Médecin Ordinaire de l'Empereur, &c., &c. Paris: Victor Masson & Fils. Pp. 206.

Collection of Memoirs concerning a Function of the Pancreas Little Known, viz, the Digestion of Azotised Aliment. By LUCIEN CORVISART, Physician in Ordinary to the Emperor, &c., &c. Paris. Pp. 206.

On the Nature, Cause, and Treatment of Tuberculosis. By HORACE DOBELL, M.D.; Member of the Royal College of Physicians, &c., &c. John Churchill & Sons. 1866. Pp. 83.

FROM the titles merely of the works which are here placed together

one might not suppose that there was any bond of union between them.

The first is a reprint of memoirs on a certain function of the pancreas. The second, also a reprint, is an exposition on the treatment of tubercular disease by means of fatty matter emulsified by the aid of the pancreatic fluid, or rather pancreatine.

The former has to do with the pancreas as an organ which prepares nitrogenised matters for digestion; the latter, with the functions of the same organ in preparing fats for assimilation.

Dr. Corvisart's book gives his researches from the year 1857 to 1863; he has, therefore, added considerably to what he had done when his views were put forward in a very able analytical review published in this Journal in November, 1858. Our readers may remember that the review alluded to elicited from Dr. Brinton a letter, published in this Journal, August, 1859; and the additions which M. Corvisart has made to his former memoirs is chiefly with a view to refute Dr. Brinton, and those physiologists who do not agree with him in attributing to the pancreas an important function as regards the digestion of nitrogenous substances.

It would be quite foreign to our purpose to enter at length into a discussion of the question at issue. With every intention to be honest and truthful the most careful physiologists will often fall into error; and when, therefore, a discrepancy exists as to the actual results of experiments, we must, in the main, be determined by the weight of evidence on one side or the other.

Now, with regard to the functions of the pancreas in digestion, it may be said that physiologists are pretty well agreed in accepting the conclusions of Professor Claude Bernard, with reference to the action of pancreatic fluid on fatty matters; with regard to M. Corvisart's view that this fluid has a powerful solvent action on albumen, converting it into peptone, matters are, however, quite different. At the conclusion of his preface, M. Corvisart gives a letter from M. Longet, in which the latter expresses his conviction of the accuracy of M. Corvisart's researches.

With this exception, however, we are not aware of any other physiologist of note who does not look on M. Corvisart's assertions as "not proven," while those who, like Keferstein, Hallwachs, and Brinton, have made the subject one of careful experimental research, have obtained results differing from his. In the letter already mentioned* Dr. Brinton gives what appears to us to be a very

* Dublin Quarterly Journal, August, 1859.

satisfactory explanation of the want of accord between the experiments which have been undertaken to test the solvent action of fresh pancreatic juice on hard-boiled white of egg. In his work, more lately published, on *Food, and its Digestion*, he does not materially modify the opinion formerly expressed. He there says, of the pancreatic fluid, that its action on the various protein compounds has not yet been established. But there are striking facts which vaguely indicate some important relation between this secretion and these constituents of the food. It is true that the fresh pancreatic juice and the infusion of the gland substance in all stages of digestion are quite incapable of dissolving albumen. But both the secretion and infusion of the gland acquire this power so rapidly in their progress towards decomposition, and in advanced stages of this process, dissolve albumen in a quantity and with a rapidity so closely akin to the copious and energetic solution of albumen effected by the secretion and infusion of the stomach, as to promise the discovery of some valuable physiological clue to both these processes of solution as the reward of future research. The more so that (as Dr. Corvisart has pointed out) the resulting solution has many of the reactions of peptone; and that the solution of albumen in this manner is specific to the pancreas, in the sense of being utterly absent even from those salivary organs (parotid, submaxillary, sublingual, &c.) which are in other respects most closely allied to the pancreas, both in structure and function. Nevertheless, the distinction between the putrid and unstable solution into which the secretion and infusion of decomposing pancreas convert albumen, and the perfectly fresh and stable peptone of the gastric juice, as well as the contrast of those circumstances which conditionate the act of solution itself, justify our denying the claims of the pancreas to rank as a solvent of albumen and other protein compounds in the healthy digestion of the living body until it is shown how these circumstances in themselves, generally so injurious to health and life, and, therefore, conclusive against the normal character of the act which involves them, are modified or prevented during the ordinary action of the pancreas as an agent in the digestive process.

Dr. Dobell's little volume is a reprint of papers which have already appeared in the *British Medical Journal*, and the *Lancet*. It consists of two essentially distinct parts—one giving his theory of tuberculosis and its treatment; the other containing reports of

cases treated with pancreatic emulsion—that is, fat emulsified by pancreatic juice.

Dr. Dobell's hypothesis is as follows:—

“Tuberculosis is due to defect in the action of the pancreas on the fat taken as food (especially the solid fat). The supply of properly prepared fat is cut off from the blood : 1, by the fats not being brought into a proper condition by the pancreas ; 2, by loss of absorbing power in the small intestine, due to the contact of unhealthy pancreatic juice and of defectively prepared food with its mucous membrane. Thus, the blood becomes deficiently and defectively supplied with fat-elements from the food ; is unable to afford those required for direct combustion ; does not replace those taken up during interstitial nutrition ; but, on the contrary, takes up more to compensate the deficient supply from the food. This having gone on up to a certain point, the fat-elements of the albuminoid tissues are seized upon, and these tissues are minutely disintegrated in the process. This disintegrated albuminoid tissue is nascent tubercle ; and this process of disintegration is tuberculisation.”

According to the theory thus put forward pure tuberculosis commences when fats properly acted on by the pancreas cease to pass in normal proportion into the blood ; and tuberculisation commences when albuminoid tissue is abnormally seized upon for its fat elements. It will, no doubt, be a novelty too startling to many of our readers at once to merit acceptance, to learn that the pancreas is the all-important organ, the functional derangement of which lays the foundation of all those terrible scourges of humanity—tubercular diseases. True to his theory, Dr. Dobell thus lays down his principles for the successful treatment of tuberculosis:—

“If tuberculisation, even in the smallest degree, is going on, the first and most urgent need is to supply fats. Olein should be given for absorption by the portal system, thus to afford, so far as it is able, materials for combustion. But, above all, **FATS ACTED UPON BY PANCREATIC JUICE** are called for, and must be supplied until the deficiency is removed, the just balance restored, the process of tuberculisation stopped. Until this point is arrived at—until the balance is turned in favour of the albuminoid tissues—everything which favours the reception of oxygen into the blood, everything which increases the wear and tear of the body, everything which calls for the generation of animal heat, directly favours tuberculisation, and precipitates the patient into the very catastrophe we wish to avert.

“On the other hand, so soon as the balance has been turned in favour of the albuminoid tissues, and tuberculisation has been artificially arrested, everything which postpones the restoration of the function of the pancreas directly favours the conversion of a temporary into a permanent disease. But moderation is essential to success. Too great and too sudden a demand upon a weak and defective pancreas may only paralyse the organism in its attempts at restoration of function. It is, however, impossible to magnify the importance of periods of arrested tuberculisation, whether artificially or naturally produced. They are the bright opportunities for the permanent restoration of pancreatic function. Too often these opportunities are wasted. The patient and his friends, pleased with the marked improvement that has taken place under some plan of treatment, either continue this treatment long after its proper time and place have passed by, or give up treatment altogether, content to wait until signs of active disease return. In either case, an opportunity for establishing a permanent recovery is thrown away, and perhaps it may never return.”

Although we cannot very readily accept Dr. Dobell's theory as to tuberculosis and its principles of treatment, yet we do not mean to say that the book is without merit. We confess, however, that we have a prejudice against books of this sort. When we bear in mind that the author is also author of a paper “On the Class of Medical Literature Most Needed in the Present Day,” and when we see before us as a reprint of papers (very well suited for the journal in which they first appeared), a neatly got up little volume of about fourscore pages, not very closely printed, and the last leaves of which are devoted to “Works by the Same Author,” and opinions of the press, therefore, we cannot help asking—is this the class of medical literature needed now-a-days? When we see that among the works by the same author is one “just published,” the title of which (*On Winter Cough*) obviously addresses itself to the non-professional reader, we then think it our duty honestly to state our conviction—that Dr. Dobell's works do not belong to the class of medical literature most needed in the present day.

On the Use of the Sphygmograph in the Investigation of Disease. By B. W. FOSTER, M.D.; Professor of Clinical Medicine in Queen's College, Birmingham, &c. London: J. Churchill & Sons.

THE last few years have been characterized by the numerous additions which have been made to our methods of diagnosis by mechanical appliances; and to the list M. Marey has added the sphygmograph, for which word of many consonants pulse-writer might be a convenient synonym. The discovery was made while that eminent physiologist was investigating the laws which regulate the circulation of the blood in health. The instrument was first used, in England, by Drs. Anstie and Burdon Sanderson; but the essay before us may be regarded as the first publication likely to introduce it to British practitioners, because of the numerous observations of its uses which it records.

As it would be most difficult to understand the construction of this instrument by verbal description, Dr. Foster has illustrated his work by excellent woodcuts; and has recorded thirty tracings of the pulse in health and in various forms of disease, which are considered so valuable by the inventor of the sphygmograph that he has asked permission to embody them in a future work of his on the subject.

In the following extract we give Dr. Foster's account of the mechanism of the instrument:—

“The sphygmograph of Marey* affords us the necessary aid; and by it we can not only gain a knowledge of the finer differences which escape our touch, but also preserve for inspection a distinct trace of these delicate peculiarities. The accompanying woodcut (Fig. 1.), copied from Marey, shows us in the interior of the frame (Q R) the essential part of the instrument, which consists of a flexible steel spring (I), covered on its under surface, at its free extremity, with a convex plate of ivory (K). This ivory plate rests upon the artery to be examined, and, by virtue of the elasticity of the spring (I), exerts a certain pressure upon it. Each pulsation of the vessel raises the spring slightly at K, and the multiplication of this movement is obtained by means of a very light lever (A), which moves upon a pivot (C). The elevation of the spring is transmitted to the lever, very near to its centre of movement, by means of a bar of metal (B E), which moves round the point (E); this bar terminates

* To the kindness of Dr. Anstie I owe my first acquaintance with the instrument; and to him we are, I believe, indebted for its first introduction to a medical society in this country.



Fig 1.—Marey's Sphygmograph.



Fig. 2.—Sphygmograph applied to the arm.

in a vertical plate (B D), and is pierced by a screw (r). When the screw acts upon the spring, the connexion is established between the spring and the bar, and the movements of the spring are transmitted to the bar, and through its vertical plate to the lever. In order to insure the transmission of the movement, the plate (B D) must be in contact with the under surface of the lever; by means of the screw (r) we can arrange this, and regulate the interval between the point of the plate (B D) and the under surface of the lever. In order that the lever should not be projected too much upwards by sudden movement, and also that it should overcome any

slight friction experienced in the paper at its terminal point (A), a small spring (Y) rests upon its fixed extremity, and presides over its descent. The screw (P) enables us to regulate the amount of pressure exercised upon the artery by the spring (I). The woodcut (Fig. 2), modified from Marey, shows the instrument placed upon the arm over the radial artery in the position for use. The lever (A) is here seen to carry at its free extremity a little pen, which, filled with ink, registers its movements upon the paper which covers the plate (X Z); this plate is moved at a uniform rate in the direction indicated by the arrows, by means of watch-work placed beneath in the case (S). Ten seconds are occupied by the passage of the plate. The button (V) enables us to wind up the watch-work; and the small regulator (G) starts the plate, or stops its motion as desired. The application of the instrument I have found much facilitated by the use of elastic bands, instead of a silk lace, as recommended by Marey. These bands embrace the arm, and are hooked on to the small projecting points on the metal framework, as seen in the diagram. The addition of a pad* to the under surface of the arm renders the instrument more easy to the patient, and prevents any pressure from the bands."

The tracing consists of as many curves as there were pulsations during ten seconds; each curve consists of a line of ascent, summit, and line of descent, which correspond respectively to the flow of blood after the contraction of the ventricle, the arrival of the blood in the artery, and the subsidence of the pressure on the vessel.

We select a few of the most characteristic of Dr. Foster's tracings to illustrate the uses of the instrument:—

Fig. 1.



Fig. 1 represents the tracing which is made by the pulsations of the healthy artery.

Fig. 2.



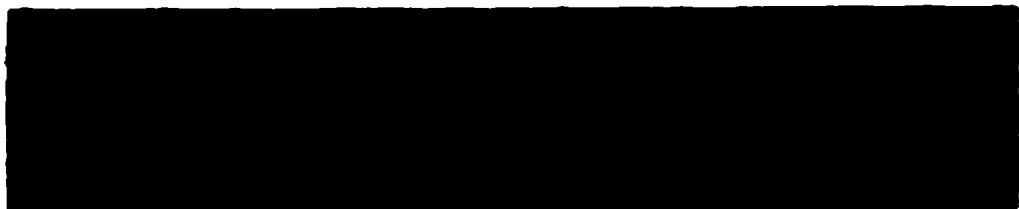
The second tracing (Fig. 2) is that made by the inelastic artery of old age.

Fig. 3.



* I am indebted to my clinical clerk, Mr. Waters, for the suggestion of the pad.

Fig. 4.



The third and fourth (Figs. 3 and 4) are those which are made by the left and right radials in a case of subclavian aneurism.

Fig. 5.



The last (Fig. 5) is very remarkable, and illustrates the tracing which is produced by the artery in cases of aortic regurgitation, which is so well known in this city by the writings of Sir D. J. Corrigan.

Among the morbid conditions, therefore, which will be in future more easily discovered with the aid of this instrument, are the diminished elasticity of arteries which occurs with senile degeneration, aneurisms on the vessels nearer to the centre of the circulation, and aortic patency; and, without doubt, as the instrument comes into extensive use so many observations will be recorded that valuable information will be afforded in the diagnosis of all diseases in which examination of the pulse by the finger has hitherto been relied on. To this end Dr. Foster's treatise much conduces, for it contains a most explicit, full, and undoubtedly faithful description of the applications of the instrument by a practical physician and an industrious clinical teacher.

New Observations upon the Structure and Formation of Certain Nervous Centres, tending to Prove that the Cells and Fibres of every Nervous Apparatus form an Uninterrupted Circuit. By LIONEL S. BEALE, M.B., F.R.S. London: 1864.

IN this memoir Professor Beale arrives at results which differ very materially from those of preceding observers. The preparations which he describes and figures, have all been taken from the ganglia of the common frog, or little green tree-frog, but the conclusions arrived at from the examination of these, are found, on the investigation of the ganglia of other animals, to be of general application,

and, hence, are of very great interest, as giving a more consistent account of the anatomy and development of this part of the nervous system, than any that has yet been offered.

The author commences by showing that apolar and unipolar nerve-cells have no existence, but that every nerve-cell is in connexion with at least two fibres, and probably influences those fibres only with which it is structurally continuous. In the frog the shape of the fully-formed ganglion cell is pyriform; near the larger extremity, or fundus, lies the nucleus, and from the smaller extremity come off the fibres.* Of these, one is straight and passes into the central part of the cell, with which it becomes continuous, while the other passes from the peripheral part of the cell, and winds, in a spiral manner, round its smaller extremity, and round the straight fibre; neither of these fibres reaches the nucleus. In the neighbourhood of the cell these fibres are pale and frequently contain masses of "germinal matter;" but in favourable specimens they can, one or both, be traced to true dark-bordered fibres, with which they are directly continuous, and hence their nervous character is clearly demonstrated; when the fibres reach the nerve-trunk in which they are to run, they separate from each other, and invariably pursue opposite directions.

In treating of the development of the ganglion cells and nerve fibres, Professor Beale gives, at some length, his theory of the nutritional and formation processes which take place in living bodies, which is now so well known as to render any notice of it here superfluous. He shows that in the fully developed frog new ganglion cells are formed in three ways:—1st. From a nucleated granular mass, like that which forms the early condition of all tissues. 2nd. By the division and splitting up of a mass like a single ganglion cell; this mode of multiplication occurs only at a period before the cell has acquired its perfect form, and prior to the development of the spiral fibre. 3rd. By changes occurring in what appears to be a nucleus of a nerve fibre.

According to Professor Beale, motion is one of the great characteristics of living matter; in accordance with this law the ganglion cell, as it grows, moves away from the spot where its development commenced; and the substance which originally formed part of the cell is subsequently drawn or spun out into the fibres. In some cases,

* In this Dr. Beale differs from Julius Arnold, who has also described the straight and spiral fibres in connexion with the ganglion cells of the frog, but traces both fibres through the cell to the nucleolus. *Journal de l'Anatomie*, ii., 220.

particularly where cells are developed according to the third way mentioned above, the fibres at first appear to proceed from opposite ends of the imperfectly formed cell; but as the latter moves away they come to lie parallel to each other, and finally, as the cell approaches maturity, the one fibre is twisted in a spiral around the other, as already described. "The spiral appears to result partly from a sort of splitting and subsequent condensation of the lower portion of the cell itself, and partly by growing from the nuclei connected with the fibres, while, at the same time, the fundus moves away, and spiral after spiral is left around the central fibre, which is of course gradually increasing in length also. I think it doubtful if the entire cell *rotates*, because the central fibre does not appear to be twisted; but it is obviously possible that the outer portion of the cell might rotate round the inner portion without causing any twisting of the fibre, the mass, of which the cell is composed, being in the natural state, very soft and plastic."—P. 14. "There is a fact in favour of rotation which I have observed so often that it may be regarded as constant, that in peripheral parts where a dark-bordered fibre is being developed, a fine fibre passes spirally around it; and this may be accounted for in precisely the same way as I have attempted to explain the production of the spiral fibre of the ganglion cell."—P. 15. As the ganglion cell grows old the proportion of germinal matter diminishes, and the number of coils of the spiral fibre undergoes a great increase.

In considering the ganglia of the heart, the author shows, in opposition to R. Wagner and Kölliker, that apolar cells do not exist, but that every cell is connected with at least two fibres, some of which certainly pass in a central direction, and that it is, to say the least, highly probable that many of the cells are in connexion with the *transcurrent* fibres of the vagus.

Many ganglia are in connexion with the nerves accompanying the arteries, and the cells of these differ in no important particular from those of the ganglia found in other parts. No artery, however small, is entirely without nervous supply. Beale confirms an observation of Luschka, that nerve fibres sometimes are found on the inner side of the circular muscular coat separated from the blood only by the very fine inner coat. He has occasion here to differ again from Kölliker, who states that many arteries are quite destitute of nerves, and who "seems to conclude in too many cases, that what he has failed to see does not exist." The following observation is important as showing that in the same nerve fibre may be

contained fibrils which fulfil very different functions:—"The nerves which supply the small arterial branches in the voluntary muscles of the frog come from the very same fibres which are distributed to the muscles. I have seen a dark bordered fibre divide into two branches, one of which ramified upon an adjacent vessel, while the other was distributed to the elementary fibres of the muscle."—P. 21.

At an early period of its development, the ganglion cell has no cell wall, but as it grows older it is surrounded by a fibro-nucleated tissue which may be described either as cell wall or matrix. This appears to be formed from the oldest and most degenerated part of the cell itself, the atoms composing it having previously existed successively as nucleolus, nucleus, and cell contents. The author's views on the relation of nervous to connective tissue are highly interesting. In true connective tissue, such as that forming the cornea, sclerotic or tendon, which "is developed as a special tissue, and may be said to fulfil a special purpose," the nerves can, by proper precautions, always be distinguished from the tissue elements, and the former never becomes continuous with the latter; but in certain indefinite forms of connective tissue such as that found surrounding nerve cells and fibres, it is often impossible to say with certainty whether an individual fibre or nucleus belongs to the connective or to the nervous tissue. It is more than probable that this indefinite tissue results from the wasting or degeneration of the nervous tissues themselves. "Nor is the nervous the only tissue which by normal wasting or abnormal degeneration, leaves what is termed connective tissue. A structure so special as a uriniferous tube, or a portion of the cell containing network of the liver, may waste, and all that represents it will be what is termed connective tissue, and connective tissue corpuscles."—P. 23.

The ganglion cells of mammalia are enclosed in a capsule of nucleated fibres. This which is generally believed to be formed of connective tissue, Beale looks on as analogous to the spiral nerve fibres which he has described in the frog. We may mention that Luys also, though taking a view of the structure of the nerve cell very different from that of Dr. Beale, combats the opinion which classes this nucleated sheath with the connective tissues, and looks on it as formed by a peculiar modification of nerve tissue, and as being an essential part of the cell.—*Recherches sur le Système Nerveux Cérébro Spinal*, p. 32.

The following passage is well worthy the attention of histologists, and with it we shall terminate this notice:—"It is possible that for

many years to come, some observers will persist in terming every thing in which they fail to demonstrate distinct structure, connective tissue, and all nuclei which are not seen in their specimens to be in connexion with positive vessels, positive nerve fibres, or other well-defined tissues besides fibrous tissues, connective tissues corpuscles; but there is little doubt that when the changes occurring during the development of special tissues shall have been patiently worked out by the use of high powers and better means of preparation, opinions on the connective tissue question will be completely changed. The idea of the necessity for a supporting tissue or frame-work will be given up, and many structures now included in 'connective tissue' will be isolated, just as new chemical substances year after year are being discovered in the indefinite extractive matters."—P. 25.

1. *The Common Nature of Epidemics.* By SOUTHWOOD SMITH M.D. 1866. London: N. Trübner and Co. Pp. 130.
2. *Notes on Cholera, its Nature and Treatment.* By GEORGE JOHNSON, M.D. 1866. London: Longmans, Green, and Co. Pp. 112.
3. *Cholera in its Home.* By JOHN MACPHERSON, M.D. 1866. London: John Churchill and Sons. Pp. 155.
4. *Cholera—its Symptoms, Causes, and Remedies.* By THOMAS A. WISE, M.D., &c. 1864. Cork: J. J. Bradford. Pp. 60.
5. *Le Choléra ou Typhus Indien Epidémie de 1865.* Par le Docteur CHARLES PELLARIN. 1866. Paris: J. B. Baillière et Fils. Pp. 72.
6. *Le Choléra et Le Congrès Sanitaire Diplomatique International.* Par le Docteur J. P. BONNAFONT. 1866. Paris: J. B. Baillière et Fils. Pp. 44.
7. *Cholera.* By WILLIAM STORY. 1865. London: E. and F. N. Spon. Pp. 79.
8. *Essay on the Nature and Treatment of Cholera and Fever, with Medical Remarks on the Treatment of Cattle Plague.* By JAMES TUCKER, M.D. 1865. Dublin: John Falconer. Pp. 40.

DURING the past twelve months cholera has again become a subject of immediate interest to European physicians, and a great addition

has in consequence been made to its literature. Some of the conclusions with regard to its propagation, its pathology, and its treatment, which were considered as most firmly established, have been questioned; these disputed views are the very ones which, if admitted, would most decidedly influence our practice, we feel called upon, therefore, to bring the whole subject before our readers; in doing so we cannot avoid repeating some facts which to many of them are well known, but we desire to dwell at length only on the important issues which have recently been raised.

The description by Willis of the disease called griping of the guts, which prevailed in London two hundred years ago, reminds us so forcibly of cholera that we are hardly justified in asserting that in former times it may not have been an indigenous disease; during the past half century, however, when it has prevailed, it appears not to have arisen in our midst but to have come to us; it has reached us from the continent of Europe, and it has reached the continent from the East; last year it appeared to originate in Arabia, under circumstances to which we will more fully allude; but on former occasions it seemed to start from India, and more particularly from Lower Bengal, which has come to be regarded as its home; certain it is that there it is always present, just as typhus fever is always present in this country; and just as typhus fever every few years assumes an epidemic form, so in Bengal, at certain intervals cholera takes on this character, and it is only at these times that it seems capable of a steady march through the rest of the northern half of the world. Now in Lower Bengal are found certain conditions which are supposed to account for the origin of a disease there which the other parts of the globe can only receive but cannot originate; through it a great river finds its way to the ocean by numerous branches; the soil is alluvial; it is so little above the sea level that the tides extend one hundred and forty miles up its rivers, and would, according to competent authorities, submerge the country but for the opposing influence of the vast body of fresh water which meets them, and as a matter of fact, they have, when propelled by a violent storm, broken over it to a distance of one hundred miles inland; over twenty thousand square miles there is nothing but swamp and forest; to the river and the sea is added another source of moisture, the periodic rains, which continue for five months, and are sufficient to saturate every inch of ground. The bodies of the dead are by a large proportion of the population committed to the river to be carried up and down by the ebb and

flow of the tide, and the dead of animals as well as every kind of excrement are left exposed on the open ground. The population in many places is dense, and the houses are merely mud cabins, without ventilation, often hidden by jungle, and in the rainy season surrounded by water. On this country for four months in the year the rays of the sun fall almost vertically, and vegetable and reptile life abounds. Amid these conditions cholera is always present, and we have described them because we are convinced that the more closely such conditions are approached in other lands the more favourable do they become for its development.

In Britain, however, our acquaintance with the disease is principally in its epidemic form; now epidemics, as such, have been studied, and it has been asserted that there are certain features, and what is more important, certain causes common to them all. Dr. Southwood Smith endeavours to prove this in the work before us; and maintains that as the black death, and the sweating sickness, and the plague have disappeared from England since land became more extensively cultivated—since some attention has been paid to the sewerage and ventilation of the towns, and since the people have been better supplied with food, clothing, and fuel, and more temperate in their habits—so a perseverance in improving the general condition of the country will ultimately cause the disappearance of the epidemic diseases which in the present century fall upon us. We do not hesitate to admit, with Dr. Smith, that there are many points of resemblance between cholera and other epidemic diseases; but there are also differences between it and them which forbid us applying unreservedly to it conclusions drawn from the observation of these, and we will therefore limit ourselves to the consideration of its features alone.

What circumstances have contributed within the last sixty years to give to it its epidemic character, we do not know. It existed in the East long before. Dr. Macpherson, in his graphic account of the disease in its home, quotes a Portugese and a Dutch physician who described it, the former at Goa, more than three, and the latter, at Java, more than two hundred, years ago. Certain it is, however, that since 1817 it has taken root in Bengal, and has since then, too, shown its capability of spreading through the rest of Asia, and to Europe. M. Bonnafont, indeed, tells us that this has arisen through the misgovernment of the English, who have allowed to go to ruin the splendid reservoirs and tanks in which, under the Mogul princes, were collected, and restrained the waters which now

overflow the country; in this we cannot agree with him. Nevertheless a change has taken place in the condition of Lower Bengal; the great tract of marsh and jungle which now harbours only beasts of prey was, three hundred and fifty years ago, well peopled;* the change, however, has been caused—not by the neglect of British rulers—but by silting of the sand at the mouth of the rivers, and the impregnation of the soil with saline matter, and its consequent destruction for agricultural purposes.

It has long been maintained that certain atmospheric conditions are necessary to the epidemic spread of cholera. The result of Dr. Macpherson's observations in Calcutta is, that "dry air, with high temperature and wide range of the thermometer, is most favourable to the development of cholera, while moist air, with high temperature and small range, is most unfavourable to it;" he found heavy falls of rain and diminished range of temperature were the most powerful agents in diminishing the prevalence of cholera; the deaths during the three hot months were, according to some of his returns, nearly as numerous as during the other nine. Dr. Morehead, on the other hand, says that his experience does not lead him to the conclusion that cholera has its preference, in India, for particular seasons; and, as he remarks, there always exists in India a temperature sufficiently high to favour atmospheric impurity—so that, if it act in this way, one could not suppose that season would exercise so marked an influence as in Europe. Many Indian medical officers have observed great heat, calmness of the air, and general oppressiveness to precede violent and sudden outbursts of cholera. Both on the continent and in Britain Summer and Autumn have, on the whole, been the seasons of greatest prevalence; but it does not appear, we think, very certain that, during the years when cholera visited us, these seasons presented unusual phenomena as is involved in the idea of an epidemic constitution of the air. According to Mr. Glashier, however:—

"The three epidemics were attended with a particular state of atmosphere, characterized by a prevalent mist, thin in high places, dense in low. During the height of the epidemic, in all cases, the reading of the barometer was remarkably high, the atmosphere thick; and in 1849 and 1854 the temperature above its average. A total absence of rain, and a stillness of air amounting almost to calm, accompanied the progress of the disease on each occasion. In places near the river, the night temperatures

* De Barros, quoted by Sir R. Martin.

were high, with small diurnal range, with a dense torpid mist and air charged with the many impurities arising from the exhalations of the Thames, and adjoining marshes; a deficiency of electricity, and, as shown in 1854, a total absence of ozone, most probably destroyed by the decomposition of the organic matter with which the air in these situations is so strongly charged.

“‘In both 1849 and 1854, the first decline of the disease was marked by a decrease in the readings of the barometer, and in the temperature of the air and water; the air, which previously had for a long time continued calm, was succeeded by a strong S.W. wind, which soon dissipated the former stagnant and poisonous atmosphere.’”

As to the recent outbreak at Marseilles, the evidence is conflicting. According to M. Didiot, the Winter of 1864–5 was moist and mild, the Spring very warm; from the 26th May, he began to observe cases of cholera, but true cholera was not officially recognized till 23rd July; during that and the two following months, the heat was excessive, the barometric pressure comparatively feeble, but liable to sudden variations, much thunder and lightning, and a southern wind. In September the temperature became oppressive, and the usual rains did not fall, and the epidemic continued; in October, however, the temperature fell, and east winds with rain set in, and the epidemic began to decline.*

The observation of other phenomena, however, besides the meteorological, yields support to the idea that there exists an epidemic constitution of the air. Dr. Southwood Smith say:—

“4. Epidemics resemble each other in giving distinct and unmistakable warnings of their approach. These warnings consist of two events: first, the sudden outbreak and general spread of some milder epidemic; and, secondly, the transformation of ordinary diseases into diseases of a new type, more or less resembling the character of the extraordinary disease at hand.

“It is a very singular fact that both in the middle ages, and in modern times, the lesser Epidemic which has generally preceded and pre-announced the coming of the greater, is Influenza.

“The history of European Epidemics from the 14th century downwards, shows that whenever a new Plague was at hand, destined to become truly European, it was preceded by a sudden outbreak of Influenza, as general as it was violent. This is exemplified with singular uniformity in the Epidemics of the 16th century—the severest epidemic period on

* *L'Union Medicale*, 22 Mai, 1866.

record. It is most remarkable that in our own day the first visitation of Epidemic Cholera was preceded by an outbreak of Influenza which resembled, in the most minute particulars, the violent and universal Influenza that ushered in the mortal Sweating Sickness Epidemic of 1517.

“So again, on the second visitation of Cholera, in 1848, it was preceded, as we have just seen, by the universal Influenza of 1847.^a

“The second circumstance, and the most instructive one, premonitory of the advent of a great Epidemic, is a general transformation of the type of ordinary diseases into the characteristic type of the approaching pestilence. Sydenham gives a graphic description of such a transformation in the character of the fevers and inflammatory diseases prevailing in London some months before the outbreak of the Great Plague. He states that this change consisted in an approximation, in several striking features, of the general type of disease, to the distinguishing characters of the Pestilence which had not yet appeared, but was close at hand.

“In 1831, in the wards of the London Fever Hospital, I observed and recorded a precisely similar change in the general type of the Fevers in London, six months before the first visitation of Cholera. Anterior to that period, fever in London, for a long series of years, had been essentially an acute, inflammatory disease, for which blood-letting and other depleting remedies were indispensable. At this period it ceased to be an inflammatory disease; it became a disease of debility, in which no one could think of bleeding; and so closely did the prevailing fever now put on the general character of the approaching plague, which was yet six months distant, that the fever into which those Cholera patients fell, who were not killed by the first stroke—the consecutive fever, as it was afterwards called—could not be distinguished from the primary fever in the wards of the Hospital when Cholera was at its height, which had appeared there for the first time six months previously, and which has never disappeared since.^b

“It is further very remarkable that the Professors of Veterinary Medicine and Surgery in London noted at the same time a similar change in the type of the diseases of the lower animals—horses, cows, sheep, and all domestic creatures;—a change requiring a similar modification of the remedies which they had been in the habit of using.”

The majority of French physicians seem indisposed to admit the existence of a peculiar epidemic constitution of the air. M.

^a It may be remarked that for some time prior to the Cattle Plague in the Autumn of 1865, the disease called *pleuro-pneumonia* had extensively prevailed among the herds throughout the country.—[Ed.]

^b This was written in November, 1865.

Tueffard says:—"It is pretended that the Indian pestilence imprints its character upon all the affections which appear at the same time, but numerous facts, observed with attention, appear to me to demonstrate the contrary; thus, the village of Erincourt suffered cruelly from an epidemic of very severe dysentery without admixture with cholera at a time when all the neighbouring localities were ravaged by the Asiatic collapse." In the same number of *L'Union Medicale*, M. Pellarin^a states that, although during the Summer of 1865 diarrhea prevailed, as it always does, in very hot and dry Summers in Paris; it did not present any features which would justify us in considering it a diluted product of the epidemic cause; and in the number for October 7th, he informs us that he had charge of a garrison of sixteen hundred men in 1849 at Givet, while the cholera was pursuing its course in several towns in his immediate neighbourhood, and that up to the night when the disease in its pronounced form appeared among the men, there was nothing in their health to show its approach.

The former of those authors, in the article from which we have quoted, thus sums up the difficulties in admitting the existence of an epidemic constitution:—"How can we admit that constitution, which implies the idea of a general influence, could be so different in places very close to one another, that it multiplies its victims in one side of a street, while it entirely respects the other? How could we explain that it only arrives in a locality after being exhausted, or almost so, in another which is distant? Why does it spare the intermediate places? Why does it leap over great spaces? Why does it follow by preference the lines of travel? Who can conceive that a medical constitution which set out from India, could have fallen upon us tardily in the middle of a course which lasts several years."

What evidence as to the portability of cholera is afforded by the recent epidemic? Annually Mahomedan pilgrims in great numbers visit Mecca and Medina, and it is now believed that cholera always appears among them. Last year was one of special importance in their calendar; and from all countries, where the faith of Islam prevails, unprecedented numbers flocked to the Holy Cities. Facilities which they had never before possessed were given them by English and French companies, by these from India, and even more distant portions of the East, crowds of worshippers

^a *L'Union Medicale*, 3 Octobre, 1865.

were brought; those coming from the West, from Turkey and Syria, had to pass through Egypt; from thence two routes were open to them, the old one through the Desert, and the new one by the Red Sea. It is said that 200,000 assembled at Mecca, and that more than a million of camels and sheep were sacrificially slaughtered; every species of filth and the bodies of these animals were left exposed to a burning sun. The pilgrims themselves were exhausted by the privations and fatigues of the journey. Cholera appeared among them in the beginning of May, whether it had come with the Indian pilgrims, or not, cannot be known; a vast number, variously estimated from twenty to 100,000, died around the Holy Cities; and now they set out on their homeward journey. It is thought that if they had been shut up to the old route through the Desert, they might in the course of their long journey have lost it, but they crowded into the steamers and were carried to Suez. The first returning steamer reached Suez on the 19th May; some of her passengers had died during the voyage; on the 21st, the Captain and his wife died; from that date to the 1st of June, about 20,000 pilgrims were disembarked and carried rapidly by rail through Egypt to Alexandria, a city surrounded by conditions very closely approaching those of Lower Bengal; here, near the great Mahmoudieh canal they formed an encampment. It is said that the stagnant water-courses of the Delta were at this time rendered specially offensive by the carcasses of the animals which the murrain of the previous Winter had carried off. On the 2nd of June a man, who worked near the sluices of the canal, died of cholera. On the 5th there were two deaths. On the 12th the disease was officially declared epidemic. To any one who has seen the flags of all nations in the harbour of Alexandria, it will be evident that if cholera is portable, no spot could afford greater facilities for its diffusion; and so it was at Constantinople on the 28th of June, at Ancona on the 15th of July, at Trieste on the 25th; into the details of its arrival at these places we cannot enter. Between Alexandria and Marseilles there is constant intercourse, and a case of cholera occurred there on the 28th June. A report of the outbreak has been drawn up by Dr. G. Buisson, and though some of his conclusions, and even some of the facts have been questioned, the report has been adopted by the Medical Society of the city. He says:—

“Although the medical constitution of May and June was characterized by an extreme rarity of diarrhea, the presence of a crowd of

men of exotic physiognomy and strange costumes, brought the news of the ravages which cholera was making at Alexandria and at Cairo." "The first patients received into the civil hospital were all, without exception, sailors and strangers—the sailors did not necessarily belong to a ship coming from an infected country; they were, for the most part, men of all countries, who had been in the port fifteen or twenty days. At this period of the epidemic in Marseilles the cases of cholera were generally hatched in the docks or the streets near the docks."

The conclusions of the report, in so far as they refer to our present question, are these:—

"The cholera which burst upon Marseilles in 1865 was brought from Alexandria. Good quarantine measures, rigorously administered, would have been able to hinder the importation of the malady."*

With these conclusions, on the whole, we are disposed to agree; the weight of evidence collected during previous visitations tends in the same direction; communication is now so rapid and constant that we could hardly expect to find an outbreak of cholera anywhere, the origin of which, through human intercourse, is not, at least, possible; that it may sometimes travel by other means has, we think, been proved by the outbreak in the island of St. Kilda during a former period of prevalence.

Some, then, think an epidemic constitution is the parent of cholera, some that its seeds are carried by human intercourse; while others, again, believe that even though its germs are so carried, they are inoperative unless the favouring atmospheric conditions are also present; but of still greater practical importance than either, is the consideration of those conditions to which the term localizing causes has been applied.

Not only have regiments lost cholera by moving their camp, not only have persons with cholera on them arrived in cities and died there without the disease spreading; but even in cases where it does become epidemic there is the greatest difference between the rapidity of its diffusion; it had extended so widely in Alexandria in ten days that the government were obliged to take official notice of it; while at Marseilles, though the first case occurred on the 28th of June it was not till the 1st of August that it began to prevail alarmingly; and it never there reached the proportions it did at Alexandria and Constantinople. We will enumerate some of the

* *L'Union Medicafe*, Mai 22, 1866.

circumstances which seem thus to influence its spread with such proofs as we possess of their power.

We are anxious that our readers should have a well-founded conviction of the potency of these influences; such a conviction as spreading from them to the general public may have a practical result.

Foremost among them appears to be the presence in the air of fecal impurities; the atmosphere may be loaded with much that is offensive without apparently strongly influencing the disease: it is only where there are excrementitious impurities that the effect is marked; this subject was laboriously investigated by Dr E. H. Greenhow, from whose valuable paper in the *British and Foreign Medico-Chirurgical Review* for April, 1851, we make the following extract, because it deals with facts, and because it points out in detail the various modes by which contamination of the air may arise.

“We have personally taken much pains to investigate the precise conditions which, from their more uniform co-existence with it, might be supposed to produce or to aggravate epidemics of cholera. The result of our observations has been that an atmosphere impregnated with the products of fermenting excrement is at once the most obvious and most constant concomitant of cholera. These exhalations were often found, even in a concentrated form, in houses where the existence of any palpable cause of insalubrity would scarcely be suspected, and thus the fact that the pestilence sometimes passing over slums and rookeries, knocked at the door of the comfortable annuitant or the wealthy tradesman, is readily explicable. During the epidemic of 1854, in a portion of the parishes of Chelsea, and of St. Saviour's, and St. George's, Southwark, exclusive of cases in which the notes made at the time of inspection are not explicit, we personally examined into the sanitary state of the houses occupied by 392 families, in whom deaths from cholera had occurred. Out of 2,701 persons 616 had cholera, besides 871 cases of diarrhea. Four hundred and fifty of the cholera cases proved fatal. The inquiry extended alike to the dwellings of wealthy residents in good streets, as to those of lodgers in the most overcrowded and filthiest alley. Without devoting more space to the subject than we can afford, it would be impossible to convey the evidence in its fulness as it came before us, and tested as it was in every manner that we could think of, but an analysis of the numbers above given shows that the existence of the products of the peculiar decomposition alluded to were evident to the senses at the time of visit in 213 houses. In nineteen of these there were cesspools situated either below the house itself, or in such close proximity thereto that the soil had percolated through into the subsoil below the dwelling. In 220

cases, open privies were either erected against the main wall of the house, or so near to the back entrance as to allow of the emanations from the soil being observable within doors. In seventy-seven instances branch drains of imperfect construction, having direct communication with the common sewer, passed underneath houses ; or a foul, open ditch ; the main-sewer, or a principal branch, in a ruinous condition, was so near to the house as to influence its internal atmosphere. In ninety-two of the houses were untrapped sinks or drains, connected directly with the street sewer, by which the foul sewer exhalations were conveyed to the internal atmosphere. Of the entire number of houses, in only thirty-three were no sources of this atmospheric contamination detected. Not to dilate further upon the precise manner in which the air breathed by the inmates became corrupted with this foul impurity, let it suffice that persons appeared to suffer in proportion to the contamination of the air they breathed by the "privy odour," and that immunity from this appeared to secure immunity from cholera. Strong confirmation of the opinion that the effluvia from decomposing cesspool soil are capable of inducing diarrhea, is also afforded by a fact which came under our observation in the Summer of 1855. Being requested by the General Board of Health to investigate certain alleged outbreaks of choleraic disease in the metropolis, we found that there had been a general outburst of diarrhea in three or four contiguous streets of Bethnal-green, shortly after the opening of the ground for the construction of sewers, and the consequent disturbance of several cesspools. Whilst the upturned soil was exposed to the air, the atmosphere of the affected streets, never remarkable for its fragranciness, was excessively offensive, and to this cause the prevalence of diarrhea was attributed, both by the residents and by several official visitors of the district ; an opinion much strengthened by the disappearance of the disease as soon as the work was completed, and likewise by entire immunity from similar disease enjoyed by the inhabitants of surrounding streets precisely the same in character to those affected, but in which there had been no disturbance of the soil."

Several considerations also go to prove that the great liability of low lying districts to cholera, as pointed out by Dr. Farr in his report on the Epidemic of 1848-49, finds its explanation in their more frequent contamination by local impurities.

The influence of the use of water rendered impure by contamination with sewage and with saline matter, was, owing to the opinions brought forward by the late Dr. Snow, subjected by the General Board of Health to an investigation which possesses the weight of a crucial experiment, both on account of the care bestowed upon it, and the number of persons from whose cases the conclusions were

drawn. These persons lived in the same neighbourhood, were in the same position, and of the same habits in every respect but one; some of them drank water supplied by the Southwark and Vauxhall Company, others that supplied by the Lambeth Company. Now during the epidemic of 1848-49 both these companies were pumping an impure water; and there died among the tenants of the Southwark and Vauxhall Company 118 persons in every 10,000. The Lambeth water was still more impure, and there died among those receiving it 125 in 10,000. When the visitation of 1854 came the supply of the Southwark and Vauxhall Company had become even worse, and though the epidemic was on the whole a milder one than the preceding, the mortality among their tenants increased to 130 in 10,000. On the other hand, the Lambeth Company were now giving tolerably pure water, and their mortality fell to 37 in 10,000.*

The influence of overcrowding in intensifying the cholera poison has been equally proved to demonstration.

“In the workhouse of Taunton there were 276 inmates. In some of the rooms the breathing space was not more than sixty-eight cubic feet. Cholera swept away 60 of these inhabitants in less than a week.

“In the county jail of this same town, the breathing space allowed to each prisoner ranges from 819 to 935 cubic feet. Not a single case of cholera, nor even of diarrhea, occurred among the prisoners in this jail.

“The town’s people also escaped, while in the overcrowded workhouses, 22 per cent of the total number of the inhabitants were swept away.

“In the village of East Farleigh, near Maidstone, 1,000 persons were assembled for hop-picking. They were lodged in sheds, and had about eighty cubic feet for breathing space; in a few days diarrhea became universal among them; ninety-seven were attacked with cholera, and forty-six died. In the same village, at the same time, under another employer who had provided proper accommodation for his labourers, there was a complete immunity from the epidemic.”^b

These three—impure air, impure water, and over-crowding—seem in civil life, at least, the conditions which most contribute to the diffusion of cholera, and they are the ones to the removal of which we may with most hope of success address ourselves; other influences, though less susceptible of proof, are doubtless not without

* See Dr. Greenhow’s Paper, B. & F. Medico-Chir. Review, Jan., 1857.

^b Dr. S. Smith, *op. cit.*

effect—fatigue, mental depression, intemperance, bad food, or the want of good food, prolonged exposure to the sun or to chills at night, have each appeared to contribute as causes of individual predisposition to the spread of the epidemic. Impure air and overcrowding do not seem to act, as has been sometimes supposed, on the persons, by weakening their power of resistance, but on the disease, by favouring the multiplication of its germs, for it has been noticed that persons coming suddenly within their sphere are specially liable to be attacked; new arrivals and travellers, Dr. Macpherson tells us, form a considerable proportion of the attacked in Calcutta. Whether previous ill-health predisposes has been doubted. Professor Alison^a considered the phthisical and other invalids in whom a febrile action existed were little liable to cholera; but M. Mesnet^b mentions phthisis as existing in several of his cases. In other French reports pregnancy and the debility of convalescence from acute rheumatism are specially alluded to as predisposing conditions; among the insane in asylums the ravages from cholera have generally been great.

We have spoken of cholera as portable, we have avoided calling it contagious; we find in the French reports, especially those by M. Pellarin,^c M. Foissac,^d and M. Bucquoy,^e many striking examples of the disease appearing in localities, previously free from it, immediately after the arrival of a person affected with the disease, in its grave form, or with choleraic diarrhea, and in one case after the arrival of a man from a village where it was raging, though he himself remained in good health;^f and we find that the first case in these instances occurred in the houses where the contaminated had been received. Professor Alison^g records a case where it was clearly to be traced to the importation of the clothes of a person which had died of it. In the Report of the India Cholera Commission of 1861,^h it is recorded that a district, lying within the Himalayas, isolated by every natural boundary, was invaded by cholera immediately on the return of some natives to it from the plains below; we cannot, therefore, avoid the conclusion that

^a Brit. and For. Med. Chir. Review, January, 1854.

^b Archives. Générales de Médecine, Février, 1866.

^c L'Union Médicale, Oct. 7, 1865.

^d L'Union Médicale, Oct. 31, 1865.

^e L'Union Médicale, Nov. 28, 1865.

^f L'Union Médicale, Oct. 3, 1865.

^g Brit. and For. M. C. Rev., Jan. 1854.

^h Quoted by Goodeve, in System of Medicine.

cholera is portable; but if we be asked whether cholera—*when prevailing in a city*—is more liable to attack those who are brought into contact with the sick than those who are not, we are not prepared to say that it is. Morehead,^a Macpherson,^b and Goodeve,^c all eminent and observant physicians, who have lived among cholera cases, state, as the result of their own observations, that its contagious power must be feeble. In the great Indian epidemic of 1861, not a single medical officer, and few of the hospital establishments, were attacked—a record very different to that left by an epidemic of typhus in Ireland. “It does not appear,” says Professor Clarke,^d “that physicians who are attending cholera patients are more liable to the disease than others; indeed, physicians seem to possess more than usual immunity. The number who have died in this city, in the various epidemics, is small. I have no statistics at hand to guide me, but I am confident that the percentage of mortality is less than among persons in the same social rank unconnected with the profession.” On the whole, however, we think that the records of past epidemics here, and of the present one in France, have tended to show the somewhat greater liability to attack of these brought into contact with the sick, especially if not scrupulous as to cleanliness, or overpowered by fatigue, or other cause.

We have heard it stated, though we cannot at this moment remember our authority, that in one of the epidemics in Edinburgh, the nurses in the cholera ward were attacked quite out of proportion to those in other wards; on investigation it was found that feeling the depression of alarm, and having an almost unlimited supply of brandy for the patients, they had used it intemperately themselves: means were taken to prevent this, and their special liability was no longer observed.

The interest of Dr. George Johnson's work lies in the views of the pathology of cholera which he advances; his character as a scientific and accomplished physician, and the valuable additions which he has already made to our knowledge, entitle any opinions he may bring forward to our most careful consideration. Those he holds on cholera are in some respects not new, but have never been so fully developed nor so powerfully supported. The

^a Diseases of India, first ed., Vol. i., p. 867.

^b *Op. cit.*, pp. 20-21.

^c *Op. cit.*

^d New York Medical Record, May 15, 1866.

commonly-received theory of the action of the cholera poison is so clearly stated by Dr. Macpherson, that we gladly adopt his words:—

“The most characteristic symptom of cholera without doubt is the immense evacuation of fluids from the alimentary canal. This liquid was supposed to be analogous to the serum of the blood; and whether it was or was not so, it was easy to see how the blood, deprived of its water, circulates badly and ends in stagnating, not only in the capillaries, but by degrees in the large vascular trunks: thence the cutaneous cyanosis. To repair the loss of fluids by the blood, there is a general imbibition from all the tissues and structures, which end in draining themselves; the result of which is a rapid shrinking of the parts observed, especially in the fingers, nose, and about the eyes. Absorption, and the functions of the secreting organs, are suspended from the want of water, which is indicated by the intense thirst of the patients, and their demands for water. The pulmonary capillaries no longer admit a blood that has become too thick to circulate easily in them: hence their bloodless state; hence also the black colour of the arterial blood, which has not been aërated, the expired air containing more than the usual amount of oxygen, which was therefore not absorbed and converted into carbonic acid, as under ordinary circumstances.—(Davy, Rayer, Twining.) The imperfect aëration of the blood explained the defective calorification and the cold that resulted from it. The absence of blood from the lungs and the imperfect aëration, accounted for the feeling of suffocation in the more advanced stages, and the feebleness of the voice was probably caused by the weakness of the respiratory organs, which are not able to pass into the larynx a column of air strong enough to produce the natural voice.”

According, then, to the most generally-received opinion, increased discharge from the gastro-intestinal mucous membrane is the most important morbid result, since through it are produced the remaining symptoms; and however varied in their details, the arrest of this discharge is the object aimed at by almost every method of treatment—the theory and the practice are alike disputed by Dr. Johnson; the grounds on which he questions them are explained with a perspicuity to which no summary of his views can do justice; but his argument may be thus stated:—It is the testimony of the most competent observers that the degree of choleraic collapse, its intensity and fatality, is not proportioned to the amount of vomiting and purging; the phenomena of collapse, when analyzed, are very different to those of syncope from loss of blood or its constituents. The effects which have followed venesection in many cases of collapse, and the fact that in the majority of cases vomiting and purging

still continue when the collapse is passing off, disprove the idea that it is due to the draining away of the serous portion of the blood. Moreover, according to our author, the statistics of his treatment by castor oil during the epidemic of 1854 go to show that arrest of the disease in its preliminary stage was constant, and recovery from it is at least as frequent as under other methods of treatment—results clearly incompatible with the commonly-received notion; it is certain that the cholera stools contain some poisonous material; therefore we are justified in inferring that just as the virus of small-pox escapes through the skin, so that of cholera seeks its exit by the gastro-intestinal tract. The symptoms during life and the appearances found after death—distention of the *venæ cavæ*, of the right side of the heart, and of the pulmonary artery and its branches, comparative emptiness of the pulmonary capillaries, pulmonary veins, left cavities, aorta, and arterial system generally—justify the conclusion that “during the state of collapse, the passage of blood through the lungs, from the right to the left side of the heart is, in a greater or less degree, impeded.” “I believe the true explanation of the arrest of blood in the lungs to be this. The blood contains a poison whose irritant action upon the muscular tissue is shown by the painful cramps which it occasions; the blood thus poisoned excites contraction of the muscular walls of the minute pulmonary arteries, the effect of which is to diminish, and in fatal cases entirely to arrest, the flow of blood through the lungs.” The purging is an effort of nature to eliminate the poison; we should, perhaps, encourage it, or at least give medicine to cause the bowels to expel the peccant matters thrown out from the mucous membrane; certainly we should not attempt to restrain it; the smaller percentage of deaths during the earlier Indian epidemics has been attributed to the malady being then milder in its action. Dr. Johnson is inclined to attribute it rather to the use at that time of purgatives in the treatment—this is the main line of argument by which his theory is supported; but many interesting details are brought to its defence, for which we must refer to his work.

Now we are convinced that the propriety of the treatment founded on these views can only be settled by experiment, and, to a considerable extent, the results of treatment will influence our belief in the theory; much can be advanced in its favour and much can be urged against it; in medical reasoning we are obliged to assume as strictly accurate facts many things which are only approximately

such; in the present state of our science we can seldom go beyond predicting what is probable; we do not know all the conditions of any pathological process so thoroughly as to be able to do more; to experience, and to experience alone, our appeal must lie. According to Dr. Johnson satisfactory results have followed the use of castor oil. The Treatment Committee of the General Board of Health, in their report, state the mortality under the use of eliminants was higher than under any other plan of treatment. On the other hand, many of the French physicians arrived at the conclusion, in the last epidemic, that it was judicious to commence the treatment by an ipecacuan emetic, and, in the children's wards, under the direction of M. Barthez, it was found desirable, unless the tongue was clean, to give both an emetic and a purgative dose of sulphate of soda before stimulants and astringents.

Closely connected with this is the question of premonitory diarrhea. Does it frequently exist? Is its neglect followed by the supervention of the algide symptoms? Certainly the majority of those who have written on previous epidemics, have strongly insisted that this is the case. A correspondent in the *Medical Times and Gazette*, for July 14, of this year, furnishes us with some testimony, afforded by the recent severe outbreak at Amiens.

“On one point all were unanimous—that in the great majority of cases the cholera was preceded by diarrhea; that this diarrhea, if continued, would lead to the algide cholera, and that it must be suppressed.” M. Barth, as quoted by Prosper de Pietrá Santa, in his report to the Medico-Chirurgical Society of Paris,* among cases treated at the Hotel Dieu, concluded that in 95 in every 100 cases the attack commenced with looseness of the bowels, and in 75 of these the looseness preceded the other symptoms by an interval varying from one to eight days. We will presently give a description of this premonitory diarrhea, which we have carefully condensed from an elaborate paper on the subject, by M. Chauffard, in which its relation to the algide symptoms and its treatment are stated in a manner which we think would be concurred in by the majority of French physicians who have seen much of the present epidemic. As to its great frequency he expresses himself positively. It is true, as Dr. Johnson states, that some of the worst cases died with little vomiting or purging, but, as has been pointed out by Professor

* L'Union Medicale, Mai 24, 1866.

Flint,^a this is not incompatible with the assumption that transudation from the mucous membrane may have contributed to the fatal result, though it has never been passed out of the canal by vomiting or purging; fluid, in quantity, may, nevertheless, have been effused and lost to the blood; or we may suppose that in some cases, though the water effused may be small in quantity, it may have carried with it very important constituents of the blood, while, lastly, to some persons a small loss may prove as injurious as a copious discharge would be to others.

Professor Clark,^b in the second of a course of lectures on cholera, which he has been delivering in New York, throws some doubt upon the anatomical facts, on which Dr. Johnson's theory rests; but we think there can be no question that the majority of the best observers have described the lungs as singularly bloodless, and the right cavities of the heart and the pulmonary artery as distended with blood. Arrest then takes place in the smaller branches of the pulmonary artery, if we consider that inspissation of the blood does not sufficiently account for this we must believe it arises from spasm of the muscular fibres produced by such an alteration in the blood as renders it irritating; but whether this is brought about directly by the action of the cholera poison on the circulating fluid itself, or is a secondary result of the intestinal flux, is quite an open question. The cramps, Dr. Johnson thinks, show the irritant effects of the cholera poison on the muscular tissue; such may be their mode of production, but they occur in other diseases affecting the gastrointestinal tract, notably in irritant poisoning, and may, it appears to us, be with equal reason referred to some form of reflex action.

One of the arguments, which we think Dr. Johnson puts most forcibly, is that, since neither in their symptoms, nor in the effect of remedies upon them are hemorrhagic syncope and choleraic collapse alike, the latter cannot be due to the draining away of serum; on this, Flint, in the paper already quoted, well observes that diminution of the quantity of the circulating fluid, and profound alteration in its constitution, from the loss of certain of its elements, are such different conditions that we should not look for any resemblance between them, and to deny that collapse is due to loss of serum, and the constituents which drain away with it, because it does not produce the same symptoms as hemorrhage is illogical.

The effect which has not unfrequently followed venesection in

^a New York Medical Record, May 1, 1866.

^b The Medical Record, New York, April 2, 1866.

cholera is adduced by Dr. Johnson in support of his views; on this point opinions are conflicting, and we gladly quote Dr. Macpherson's experience, particularly as we think it is a remedy which should not be quite forgotten.

"When I arrived in India in 1840 I found venesection was the common practice in Calcutta; and in an epidemic in 1842 I gave it a fair trial in a good many cases which I had pretty completely under my own observation. In some cases, even when the pulse was not quite gone, only a few drops, or at most an ounce or two, could be got to flow. All such cases, and even one in which I got eight ounces, died. In other cases, when venesection was tried early and blood flowed freely, there was an immediate feeling of relief about the precordia, the action on vomiting, or purging, or cramps, being however uncertain. No case in which blood flowed readily died; but then it is to be remembered that venesection was not the only remedy employed, and that, if blood flowed freely, the case was one that had come early under treatment. Still, the general result was, that bloodletting before the pulse was gone, and still more while the pulse was good, was a valuable remedy. I also found that blood-letting was better adapted for the European than for the native constitution. In all this I was arriving at the same conclusions as Mr. Jameson, showing that, in the course of twenty-five years, there was no material change in the effects produced by blood-letting. It must be remembered of this, as of other remedies, that their applicability depends much on the character of the particular epidemic, blood-letting being most useful where the disease sets in most rapidly with threatening of collapse: most applicable to the cases of strong healthy men; not at all so to those of men already reduced by previous disease. In spite, however, of no actual dissatisfaction with the results of this experiment, I gradually gave up bleeding. Its employment requires much discrimination. If employed too late it is a depressing agent—that is, if blood be got to flow. On the whole, we do better to avoid it, although it is worth trying in cases where it is too late to attempt much by mere medicines."

We attach little weight to the argument, that vomiting and purging, being efforts of nature, should be respected. We cannot agree with a recent reviewer of Dr. Johnson's work, who ridicules the idea of curative efforts of nature. It is true our notions on this subject are vague, and we think that of late years there has been a disposition to view certain portions of morbid processes too readily as curative efforts. All we know is this, that in certain diseases, changes in function, and even in structure, supervene, changes which, if they occurred, *per se*, in a healthy person, would be prejudicial;

but, occurring in the course of disease, leave the patient better off than he was before; experience has taught us that a too ready interference with these changes is not for the good of the patient; if, as a matter of clinical experience, it is found that choleraic patients, in whom the vomiting and purging are encouraged, get on better than those in whom they are restrained, we will admit that they are curative; but let us not approach the subject with the pre-conceived notion that, being natural efforts, they must be curative.

Moreover, admitting, as we are prepared to do, that the cholera stools contain a poisonous matter, it by no means follows that the removal of this poisonous matter becomes so urgent an indication as to call for its assistance by medicines which promote vomiting and purging, which Dr. Johnson himself admits, may sometimes by their violence lead to death by exhaustion. The researches of Thiersch and Pettenkofer, render it probable that choleraic discharges are not noxious till they have undergone some change, for which atmospheric air, or the influence of contact with the soil is necessary; and if, as Dr. Johnson maintains, the analogy between small-pox and cholera is so close, there is surely no need to be so anxious to carry off the discharges, if, like the variolous matter, they are poisonous, they are not so to the patient who is already affected, but to the healthy.

As we have already said, we think the intelligent observation of cases can alone decide the propriety of the eliminative treatment; and should London unfortunately suffer, if, while the epidemic is at its height, this method is tried at King's College Hospital, we will await the result with anxiety, for we are convinced it will be fairly and impartially reported on by the distinguished physician who has so strongly recommended it. We have offered these criticisms on the theory, because the arguments in its favour have been so powerfully put by Dr. Johnson that the possibility of fallacy in his reasoning is not at first sight apparent.

Even those who may not consider themselves justified in departing so far from the orthodox treatment as to make trial of the eliminative method can assist in observing facts which bear upon the theory on which it is founded. Let it be carefully observed whether, in the majority of cases, the rapidity and depth of the collapse bears any proportion to the amount and character of the stools, and whether after death, *in the state of collapse*, the condition of the heart and lungs are such as Dr. Johnson has described. There is one other consideration which we think tends

to strengthen the generally received opinion of the gastro-intestinal loss being the cause of many of the other symptoms. It has always been surmised that in cholera there is a profound alteration in the blood, which leads to the arrest of circulation and respiration. During the last European epidemic, an attempt was made to determine the actual nature of this change, and the result went to show that it was such a change as would presumably follow the unique flux which marks the disease. Schmidt made a series of examinations of the blood, an account of which was published in the Report drawn up by Dr. Baly and Dr. Gull, and also in a review of that Report, by Dr. Parkes, in the *Medico-Chirurgical Review*, for July, 1854:—

“ In the transudation of cholera (a period of short duration = 36 hours, *Schmidt*), the intercellular fluid of the blood (*i. e.* serum and fibrine) is first affected, and water, salts, and a small portion of albumen pass off, and form the well known liquid stools.

“ The order in which the constituents of the serum are affected is thus laid down by Schmidt. The water transudes before the solids of the serum; the inorganic before the organic solids; the chlorides before the phosphates; the salts of soda before the salts of potash. The causes of this order of transudation are as little known as the cause of the transudation itself; but it is interesting to observe that the order is very much the same as takes place during the action of some purgative medicines, as elaterium.

“ Very soon after the transudation of some of the constituents of the serum commences, an important change occurs in the blood; the normal diffusion-currents between the fluids in the blood-cells and around them alter; and the constituents of the blood-cells transude into the serum in the same order as the constituents of the serum transude into the alimentary canal; that is to say, the water diffuses more readily than the solids; the inorganic solids more readily than the organic; the chlorides, and of these the soda salts, more readily than the phosphates. The result is, that at the height of the transudation period, the constitution of the blood is profoundly altered. Of course the percentage of the organic constituents (which pass off with so much difficulty) is enormously augmented; it is probable that their composition must have undergone alteration, but of this nothing is known. The inorganic constituents of the blood, if compared to the water, are at first (during the first four hours), increased, because at this time the water is passing off with great rapidity; afterwards, as the salts pass off, the disproportion

is lessened, and after eighteen hours, the proportion of salts is greatly diminished, and if compared with the organic constituents, the diminution is enormous. With respect to the individual salts, there is in the blood a relative preponderance of phosphates over chlorides, and of potash salts over soda salts. This follows, indeed, as a matter of course, if the formula for the order of transudation already given, be correct.”^a

With these results before us, it becomes difficult to resist the conclusion that the first step in the process is the transudation from the blood into the intestinal canal. The liquor sanguinis of the blood loses certain of its elements, and in virtue of the laws which operate in complex fluids, these elements are supplied from the corpuscles which float within that liquor sanguinis, thus altering the constitution of the whole, and rendering it difficult of circulation, as well as unfit to subserve respiration and calorification.

In Dr. Morehead’s work^b we find a case which we think is not without interest in this connexion. A patient was admitted to the Bombay Hospital, with effusion into the left pleural cavity and into the peritoneum, apparently connected with renal disease; he had diarrhea which passed into cholera, after the supervention of which he lived six days, the pulse not being lost till just before death, and coldness of the surface being only an occasional symptom, apparently in consequence of the blood being supplied with serum from the effusions which disappeared during the attack.

There is little doubt that epidemics of cholera, like other epidemics, differ from one another both in violence and in prominence of certain symptoms. Dr. Macpherson^c specially mentions this, and so does Dr. Wise;^d that the disease, however, has lost anything of its early intensity is unsupported by facts; Macpherson tells us that it is certainly as great as it was in 1817; from the excellent letters of M. Foissac^e we learn that the proportion of deaths to attacked has rather increased than decreased; but the proportion of persons attacked to the population in each successive epidemic seems steadily lessening. Prosper de Pietra Santa^f gives the following as the number of deaths to population in Paris:—

^a Brit. & For. Med. Chir. Review, July, 1854, p. 139.

^b Researches on Disease in India, 1st ed., Vol. 1., p. 397.

^c *Op. cit.*, p. 11.

^d *Op. cit.*, p. 8.

^e L’Union Medicale, Oct. 19, 1865.

^f L’Union Medicale, Mai 24, 1866.

In 1832	-	-	-	1 in 40
In 1849	-	-	-	1 in 51
In 1853-54	-	-	-	1 in 112
In 1865	-	-	-	1 in 270

The outbreak in continental towns seem also very clearly to bear out the truth of Dr. Farr's views, that epidemics are self-limited, that they have a period of increase, a climax, and a period of subsidence; on the whole, they also go to show the greater fatality of the seizures in the beginning of the epidemic; this curious fact, however, is stated in the *L'Union Médicale* for October 24, 1865; the rapidly fatal cases (*les cas fondroyants*) were few at the beginning of the epidemic, were frequent in the middle of it, and became less frequent towards its close; the rate of mortality to attacked varies so much in different periods of the epidemic that it is impossible to say what it usually is. At Amiens, when the outbreak was at its height, 60 per cent. of cases presenting algide symptoms died; but at present, according to the correspondent already quoted, 60 per cent. of those recover. In framing statistics, it is important to determine when does the case become true cholera. Dr. Mesnet, Hôpital Saint Antoine, fixes on cramp as the boundary symptom; in the mildest of the cases reckoned by him as choleraic, there existed epidemic diarrhea, with or without vomiting, well-marked cramps, preceded or followed by debility and *malaise*, and his figures are as follows:—

213 cases (all males).^a Cured, 126. Died, 87.

Epidemic Diarrhea 29		Cholera Leger 49		Incomplete Collapse 68		Complete Collapse 48		Dying on Admission 19	
Died	Cured	Died	Cured	Died	Cured	Died	Cured	Died	Cured
—	29	7	42	24	44	87	11	19	—

Of these 88 there died:—

In collapse without reaction,	-	-	-	25
In a state of irregular abortive reaction,	-	-	-	26
Of secondary affections,	-	-	-	13
In a typhoid adynamic state,	-	-	-	6
With cerebral symptoms (<i>dans l'état meningi-</i> <i>tique</i>),	-	-	-	4
By suicide,	-	-	-	1
Of disease contracted in hospital,	-	-	-	2

^a Archives Générales de Médecine, Février, 1866.

According to the report of M. Chauffard,^a already referred to, diarrhea was not unusually prevalent *before* the last epidemic, the first indication of which was true cholera in isolated cases, but *during* the epidemic and in frequency proportionate to the cases of confirmed cholera, he noticed gastro-intestinal ailments in such number that at one time they seemed to constitute the sole sickness of the community—these derangements grouped themselves under four heads.

1. Cases in which there were very frequent serous stools, often with some nausea—except that the secretion of urine continued, and that cramps did not come on—these cases presented all the symptoms of cholera in a mitigated form; they were liable to get worse either gradually or suddenly, and end in death.

2. Cases in which the patient has four or five stools in twenty-four hours, with hardly any feeling of illness, but at the end of two or three days a sudden attack of cholera.

3. Cases presenting the ordinary symptoms of derangement of the stomach (*embarras gastrique*) anorexia, foul tongue, clammy mouth, vertigo, but without fever or headache; these were liable to end suddenly in cholera.

4. Cases in which, after some epigastric distress, perhaps, one or two loose stools and some faintness, coldness and vertigo, profuse viscid sweats break out and recovery takes place.

For all these forms of gastro-intestinal derangement, rest in bed, abstinence from food, an ipecacuan emetic, followed, if necessary, by opium and bismuth, are the means recommended. Opiates and astringents given without being preceded by an emetic he found dangerous, the symptoms often returned and true cholera came on; sometimes the symptoms subsided spontaneously. M. Gibert draws attention to the state of the tongue, as enabling us to diagnose the epidemic diarrhea from looseness of the bowels due to ordinary causes; in the latter it is red, dry, and pointed, whereas in the choleraic form it is always large, pale, moist, white, and covered with mucous layer. By some observers, vertigo was noticed as a frequent symptom. M. Marotte^b gives it, as the result of his experience, that these cases could not be considered out of danger till some febrile reaction had followed.

We now come to consider the treatment of cholera, in its fully developed form, availing ourselves of such reports of the recent

^a L'Union Medicale, Oct. 31, 1865.

^b L'Union Medicale, Oct. 21, 1865.

epidemic in France as come from physicians who have had sufficient opportunities of treating the disease, and appear to be able with calmness and sobriety to appreciate the results.

The Indian authorities—Macpherson, Goodeve, and Morehead—are all unanimous as to the propriety of attempting to check the first symptom of cholera by a full opiate, one to two grains of solid opium, or an equivalent of one of its fluid preparations, which is probably to be preferred; if rejected it is to be repeated, and it is to be followed by small doses of acetate of lead and opium—the combination which in the first epidemic was recommended by Dr. Graves, and which seems to have yet more evidence in its favour than any other; but it had better be given in the fluid form, two to five grains, with a few minims of the solution of morphia, may be administered every hour, or even every half hour; if, after these remedies have been continued for four or six hours, the discharge continue, their inutility is manifest, and they should be abandoned. Our own practice was to give at the outset forty minims of Battley's sedative liquor, half a drachm of spirit of camphor, and the same quantity of sulphuric ether, washed down by a little good brandy and soda-water, instead of the opium above as they advise.

If we may be allowed the expression, it has seemed to us that patients not unfrequently all but recover from cholera—the struggle between the powers of life and the force of the poison is so close that if the former had held out a little longer the patient would have been saved; the same is seen in fever where judicious support during the critical hour saves a patient who, notwithstanding the care which had been bestowed upon him in the early period of the disease, would have succumbed in the end but for this little but all-important assistance; in cholera, however, those who have carefully watched for hours the course of a case must agree with Dr. Johnson that it is vain to seek by quantities of powerful stimulants to keep up the failing powers; there are no means of introducing fresh supplies to the garrison; our only hope is to economise those which are already within: for this reason, it was our habit to beg the patient to restrain, as far as possible, the restless jactitation, which is so distressing a feature of the disease. Often, after the attack has existed some hours, the motions, though frequent and watery, are not profuse, and should be caught on a folded blanket laid under the sufferer; from the first they may be received in a bed-pan, and the exhaustion of rising to stool avoided; in the same way, instead of standing up and leaning over the basin to

vomit, the contents of the stomach should be caught in a towel—all efforts to force food, even beef-tea, upon the patient we think injurious; the urgent thirst should be assuaged by small quantities of cold water or iced soda-water; if large quantities are given the vomiting is usually more frequent; cold drinks are generally urgently demanded by the patient, but we have known hot tea prove grateful, and tea, with a little rum or brandy in it, seems a favourite drink with continental physicians. We are not of those who dread stimulants on account of their producing the secondary fever, because there is abundant evidence of secondary fever in cases which have not been treated largely by them. Dr. Macpherson specially mentions this fact; they may, however, aggravate it, and so probably may opiates administered in periodic doses during collapse; equally certain, however, is it, that stimulants do not produce the effect of rousing the patient; the opinion of the best authorities, and our own observations of cases which we never left, except for a few minutes, from the outset of the disease until its close, have convinced us of this, and we mention it because it is difficult to stand at the bedside of a patient in collapse and not feel constrained to pour in stimulants in larger and larger doses; they do not restore heat nor fill the arteries, and the taking of them, and the vomiting of them again, as often happens, distress the patient. Emetics have been recommended in collapse; we have no experience of them; it is possible a rapidly acting, powerful emetic might have a good effect; certainly the teasing sickness which follows injudicious feeding and medication only exhausts the sufferer; a little good brandy, or small quantities of the aromatic spirits of ammonia, may be added to the drink, and if they appear to influence the pulse, *cautiously* continued, against the distressing vomiting, we possess no reliable remedy; fluids, if iced and given in small quantities, often do not return till they have accumulated, so as slightly to distend the stomach; a scruple dose of calomel laid on the tongue and washed down with iced water, has once or twice appeared to us to settle the stomach. Goodeve advises a large sinapism to the epigastrium, and the hypodermic introduction of half a grain of muriate of morphia; a small glass of good (not sweet) champagne is sometimes grateful; the cramps, however, can generally be controlled by friction, with turpentine or chloroform liniment; if severe, Dr. Goodeve advises the inhalation of chloroform, which, if moderate, he says, “may be used even in great debility without increasing the weakness of the pulse.” On the

continent, very violent means have been recommended for maintaining external warmth, but we doubt whether any are advisable beyond drying the body with a soft warm towel, and laying bags filled with hot sand or hot bottles, near the body; a correspondent in *L'Union Medicale* asserts that repeated injections of very hot chamomile tea, with brandy and lemon juice, thrown high into the bowels were useful in restoring heat; very hot water would probably be quite as efficacious, and is, perhaps, deserving a trial, to it might be added two drachms of Condyl's fluid; another correspondent recommends, with much assurance, that the abdomen should be covered with a mixture of collodion (80 parts) and castor oil (20).

Dr. Wise, in his little work, tells us that he has seen the heat kept up in collapse, by applying compression to the femoral artery on one side, and to the brachial on the other; the limb having been first well rubbed from below upwards, so as to favour the return of blood, which the ligature prevents re-entering the extremities. He says it may be kept on for hours, or even for a day or two.

There appears to be some evidence in favour of the use of Condyl's fluid. Dr. Howard Montgomery gives the statistics of twenty-three cases, with only one death, in which the treatment consisted of "Condyl's disinfecting fluid, internally, stimulants and counter-irritation, and nutrition." ^a Dr. Doyle ^b treated forty-six prisoners, in Cuddalore Jail, with the fluid: half a drachm to a drachm, in water, every half-hour, with brandy, broth, and frictions; but he also gave, at the commencement, two astringent pills; he had only fifteen deaths, which, considering the class of patients, and that they "were all well developed cases of collapsed cholera," we consider a very low death-rate, though greater than in the twenty-three cases in which no astringent was given; five patients who refused the treatment died.

Hot baths have been strongly recommended; and on theoretic grounds they seem such judicious remedies that we think it necessary to tell our readers that two of our most competent observers, Dr. Parkes and M. Mesnet, were satisfied that they were injurious. On the whole, then, in the stage of collapse, we think, there is greater danger of doing too much than too little; there is no disease in which the urgency of the symptoms seems more imperatively to demand energetic treatment, and the temptation

^a *Med. Times and Gaz.*, March 10th, 1866.

^b *Madras Quarterly Journal of Medical Science*, Feb., 1864.

is, indeed, strong to try one remedy after another; painful experience, however, has proved their inefficiency; above all things, then, do not torment the patient with incessant medication, try to keep up the warmth of the body by light blankets and jars, but not so as to oppress; endeavour to relieve cramp; give a little drink when desired—to it may be added brandy or ammonia in small quantities, or Condry's fluid; if it can be done without exhausting the patient the hot enema may be thrown up, and a mustard plaster may be laid over the stomach; but remember that the medicines, which the case seems to require, have already been tried in similar cases, and do not think that by redoubling the dose of the stimulants and increasing the frequency of their administration, by trying first one and then another, that you must succeed—injury, not benefit, is likely to follow such a course.

The treatment of the secondary fever should not, we think, differ in any essential respect from that of any pyrexial state presenting similar symptoms. The officious use of purgatives and mercurials are specially condemned by Dr. Goodeve as liable to cause a relapse; the secretion of urine should be encouraged by counter-irritation to the loins, and the use of the ordinary non-irritant diuretics. Mesnet considers the character of the secondary fever is determined by the idiosyncrasy of the individual; cerebral symptoms, especially where they are those of a hyperemic condition of the nervous centres, may be combatted by leeching and blisters. M. Mesnet describes, under the designation of the typhoid state, cases in which somnolence, amounting sometimes to coma, was the prominent symptom, and he thought quinine, fifteen grains in the day, was good; *L'état cérébral méningitique* he applied to a kind of drowsiness from exhaustion. English practitioners have usually attributed head symptoms to uremic poisoning. Gastro-intestinal lesion of the inflammatory kind does not appear to have been observed so often in Europe as in India, where it is treated by fomentation, leeching, and blistering.

In cholera times there are some things which should be done. The general importance of sanitary measures has been so fully pointed out by the zealous and most competent officer of health for this city, and by others, that on them we say nothing; except this, that if the cleansing of sewers and cess-pools is left till cholera is among us the disturbance of the filth will usually do more harm than good.

We think that those who have not the calls of duty to detain

them, and specially those who, having no occupation for their minds, are likely to be a prey to fears and fancies, will do well in cholera times to leave a city where it has appeared. They should not go to places at the sea-side, but to elevated situations inland. A shooting lodge on the moors is safer than a town house. There are public and patriotic considerations which ought to prevent some from adopting such a course, though free to do so; but it is, nevertheless, for themselves the wisest. If they go, let them not return till the epidemic has been for some weeks completely over, as arriving freshly within the sphere of its influence they would be exposed to greater risk.

The late Professor Alison pointed out the benefits arising from houses of refuge, where were received the unaffected inmates of overcrowded dwellings in which cholera had appeared, and if the epidemic comes we would urge their establishment. That the congregation of a number of people together, unless with singularly good ventilation, in cholera times, is fraught with danger, is a consideration with which boards of guardians should be made acquainted.

The propriety of moving patients, as soon as the violence of the attack is over, to a separate ward, has been well pointed out by Mesnet as reviving their hopes and saving them from the depressing effects of witnessing, perhaps just as they are coming out of their first sleep, others around them in the agony of cramp and vomiting. We have ourselves seen a patient, a fine athletic man, who had struggled through collapse, and had slept, so terrified by the sight of the intensely rapid death of one of his comrades, who unfortunately occupied the next bed, that he gave himself up, a conviction which was soon realized. That there should be separate wards for cholera cases seems to be the general opinion in France.

Disinfectants should certainly be used in Cholera Hospitals, and in private houses when it appears. Those about the sick will do well to rinse their mouths and wash their hands before eating, with water to which Condry's fluid has been added. Heat above 180 is a powerful disinfectant. We learn from Dr. Mapother's Report that, acting on this fact, means have been provided in Liverpool for exposing the clothes of infected persons to a high dry heat; and in all probability if towels and bed linen are immediately thrown into boiling water any noxious element attaching itself to them will be effectually destroyed. If possible, choleraic vomit and dejections should not be thrown into a common sewer, but should be buried;

in any case means should be taken to destroy any noxious matter contained in them. Common lime (one of the cheapest and best disinfectants), or the chlorinated lime, may be smeared over the inside of the bed-pan. If it seems desirable to let the room become filled with a volatile disinfectant chlorine may be liberated.*

Finally, in cholera times we do not think any food which does not disagree under ordinary circumstances should be eschewed. Let meat be fresh and well cooked; let moderation be the rule, not only in eating but in work and in pleasure. Intemperance strongly predisposes to the disease; but we cannot at all concur with those zealous reformers who recommend total abstinence in view of a cholera epidemic. To those who have hitherto indulged too freely such a complete deprivation (though under most circumstances we believe it to be the best course) would, we think, leave them specially liable to the malady; and to persons in feeble health we would recommend the moderate use of good liquor.

Our estimate of the works at the head of this article may be inferred from our references to them. Though we are not able to go quite so far as Dr. Southwood Smith, we recommend his most interesting account of the influence exerted on mortality in England by civilization, to all who have the welfare of the people at heart. Dr. Johnson's volume should be read by all who are likely to see cholera; whether disposed or not to follow his suggestions they will have their attention drawn to the points on which we are in want of accurate and impartial observation. Dr. Macpherson not only gives us a graphic account of the phenomena of cholera in Bengal, but touches with singular terseness and vigour every difficult point in connexion with the disease, and displays an acquaintance with the literature of the subject which does no little credit to one who has been so long exposed to a Chowringhee atmosphere. To Dr. Wise's peculiar treatment we have alluded already. M. Bonnafont thinks that cholera, coming originally from India, would die out *en route* if it did not meet here and there marshy undrained land, from which it is able to draw "*de nouveaux éléments de force reproductive.*" He would, therefore, have the

* "Mix eighteen parts of finely ground common salt, with fifteen parts of *finely pulverized* good binocide of manganese; put the mixture in a flask, and pour a *completely cooled* mixture of forty-five parts of concentrated sulphuric acid, and twenty-one parts of water upon it, and shake the flask; a uniform and continuous evolution of chlorine gas will soon begin, which, when slackening, may be easily increased again by a *gentle heat.*"—*Presenius.*

sanitary congress set on foot a plan for every country draining the marshes within it. For the draining of the Delta of the Ganges, on account of its magnitude, he would invite the co-operation of all civilized nations. M. Pellarin's pamphlet is mainly in support of the doctrine of contagion. Of the remaining works the less we say the better.

Observations on the Functions of the Liver, more especially with Reference to the Formation of the Material known as Amyloid Substance, or Animal Dextrine, and the Ultimate Destination of this Substance in the Animal Economy. By ROBERT M'DONNELL, M.D.; M.R.I.A.; Surgeon to Jervis-street Hospital, Lecturer on Surgery in the Carmichael School of Medicine, Dublin; Fellow of the Royal Medical and Chirurgical Society of London, &c., &c.

Recherches Physiologiques sur la Matière Amylacée des Tissus, Fœtaux et du Foie. Par M. LE DR. M'DONNELL (Commissaires MM. MILNE EDWARDS, COSTE, CL. BERNARD). Comptes Rendus de l'Académie des Sciences. Tome LX. 8 Mai., 1865. P. 953.^a

Recherches sur la Substance Amyloïde de Quelques Tissus du Fœtus, et sur les Fonctions du Foie. Par Le DR. M'DONNELL. *Journal de la Physiologie de l'Homme and des Animaux*, &c. Tome VI., p. 554.

THE subject of the above interesting and highly important memoirs, which we propose now briefly to review, has occupied the attention of our author for a considerable period of time. We find the earliest published account of his investigations upon the amyloid substance of the liver in the *Dublin Quarterly Journal of Medical Science* of August, 1859,^b and at various subsequent dates essays upon the same subject have appeared in the Proceedings of the Royal Irish Academy, the Royal Society, and elsewhere.

In the preface to his late memoir, Dr. M'Donnell puts forward the importance, both theoretical and practical, of his researches, in a

^a See also Vol. lxi., page 533—of same periodical—dated September 25th, 1865, wherein a typographical error, occurring in the article just quoted, is corrected.

^b On the Physiology of Diabetic Sugar in the Animal Economy.

fashion so clear, and withal so modest, that we believe we cannot do better than reproduce his own words:—

“The majority of those engaged in practice, still, I believe, look upon the liver as if the principal duty of this gland was nothing else than the secretion of bile. It is certain, however, that it does other work, little, if at all, inferior in importance to the formation of biliary matters, and quite as necessary to the maintenance of health. Its power of making, and storing up for a time within its cells, a material resembling starch, constitutes, without doubt, one of its most important functions. This no person will for a moment doubt, who takes the trouble of ascertaining, by experiment, the immense increase or diminution in bulk which the liver may be made to undergo in the space of a few days by such changes of diet as increase or diminish the amount of this starch-like material in its tissue.

“The subject is one which I conceive to be of great interest, in a practical point of view; and on this ground I would solicit the attention of practitioners to the *facts* recorded in the following pages, especially those relating to ‘The Formation by the Liver of Amyloid Substance’ (p. 11, and seq.). While I venture to hope that physiologists will also carefully consider the facts, I must also hope that those who differ from the theoretic view, as to the ultimate destination of the amyloid substance in the animal economy, which I advance in the following pages, will, at least, remember that I do not put forward my view with dogmatism, but as one keenly alive to the difficulties and delicacy of the question at issue.”

In order now to present to our readers a *résumé* of Dr. M'Donnell's views, and of the line of argument by which he sustains them, we shall quote freely from his introduction.

“In March, 1857, Professor Bernard announced the important discovery of a material formed by the liver, closely resembling starch, or, rather dextrine of vegetable origin, and like it readily changed into sugar in the presence of ferments. The discovery of this amyloid substance did not alter his views concerning the formation of sugar in the liver, but rather served to place his so-called ‘glycogenic theory’ on a more secure basis than before. Nevertheless, the researches of Dr. Pavy,^a to say the least, cast great doubt on the correctness of this supposition, while the beautiful observations of Charles Rouget,^b and of Bernard^c himself, on the foetal tissues, tend to show that the amyloid substance

^a Philosophical Transactions, 1860, p. 595.

^b Journal de la Physiologie de l'Homme et des Animaux, tom. 2, pp. 83 and 308.

^c Ibid., tom. 2, p. 326.

(under certain circumstances) serves a very different end from that of being thus transformed into sugar.

"Some years ago, after a careful repetition of the experiments of Dr. Pavy, I was led to agree^a with the conclusion arrived at by him, that the liver is not endowed with the power of converting its amyloid substance into sugar during life and health. If, therefore, not transformed into sugar, the question naturally arises, What becomes of it? What is its ultimate destination in the animal economy? It is my chief object in the following memoir to attempt to answer this question.

"I have elsewhere^b stated, that although the question must be admitted to be one of the greatest delicacy, yet that it appears to me, on the whole, there is evidence that the amyloid substance met with in the liver is, as it were, on its way upwards towards the more exalted or complex immediate animal principles, and that its conversion into sugar is not its normal destination; that the process of healthy assimilation tends, if the expression may be used, to promote it from the rank of ternary (hydrocarbonous) to that of quaternary (azotised) compounds, and that its conversion into sugar is, therefore, a deviation from this progressive course—a *dissimilative* instead of an assimilative process. In order to establish this view it became necessary—

"First.—To re-examine, with precision, the facts which have induced Pavy to conclude that the amyloid substance of the liver is not transformed into sugar during life.

"Secondly.—To investigate the chemical and physiological relations of the amyloid substance, not only of the liver but of other organs and tissues, and test the very interesting results which are for the most part due to M. Charles Rouget.

"Thirdly.—To compare the portal and hepatic blood with each other, and with arterial and venous blood derived from other sources, and consider the relations to each other of the different functions performed by the liver. For if it be true, as Lehmann, Brown-Séquard, and others have asserted, that the fibrine and much of the albumen of the portal blood vanish in the liver, and that, at the same time that it destroys these azotised compounds, it forms its non-azotised amyloid substance and excretes bile, containing so little nitrogen that it need hardly be taken into account; are we not, from the consideration of these functions, led to infer that the nitrogen, which leaves the liver by no other outlet, may go forth in the hepatic blood, in union with the amyloid substance thus changed into a new azotised principle? That thus the liver is a

^a On the Physiology of Diabetic Sugar in the Animal Economy, 'Dublin Quarterly Journal of Medical Science,' August, 1859.

^b On the Formation of Sugar and Amyloid Substance in the Animal Economy. Proceedings of Royal Irish Academy, February, 1860.

great blood-making organ, in which there is constantly going on a reconstruction of certain ingredients of the blood; that in it the fibrine, &c., which has done its work, is disintegrated, the hydrocarbons of the bile abstracted, and the nitrogen combined with the amyloid substance, which, instead of being normally changed into sugar, emerges from the liver as a constituent principle of the protoplasma, from the bosom of which (to use the words of Bernard with reference to the fetal tissues) organic evolution is to be accomplished."

The tendency of Dr. M'Donnell's views will readily be gleaned from the foregoing extracts. In a word his theory amounts simply to this, that the dextrine found to exist in the animal organism is *not* changed into sugar destined for the maintenance of animal heat, and consumed in the respiratory process; but that it becomes ultimately united with nitrogen, and so gives origin to nitrogenous compounds.

Dr. M'Donnell's arguments rest principally upon two fundamental points, namely, that, in the first place, as demonstrated by Dr. Pavy's and his own experiments, the conversion of the amyloid substance of the liver into glucose is not its normal destination under circumstances of perfect health; and secondly, that the proven ultimate use of this substance in the fetal tissues is not connected with the respiratory process. Dr. M'Donnell has elaborately worked out the latter problem, following up the researches of M. Charles Rouget and Professor Claude Bernard, adding to the microscopical observations of these physiologists his own original and exhaustive chemical investigations of this substance in the fetal tissues—investigations which, we think, amply justify the anticipations he thus expresses:—

"I hope to be able to show, however, that the establishment of respiration has little to say to the disappearance of the amyloid substance from the tissues of the foetus, and to prove that, in truth, certain azotised tissues are evolved from a nidus of amylaceous protoplasm, which, after a particular stage of growth, becomes less and less as each of these tissues approaches maturity, and that, when maturity is attained, the amyloid substance, which once formed so large an ingredient of the growing tissue, has gradually become changed, so as to be no longer discernible even before respiration has commenced."

In the latter pages of his memoir, Dr. M'Donnell enters upon the investigation of the differences in the blood which enters, and that which emerges from the liver, as well as upon the consideration of the functions of that viscus.

This portion of the essay—perhaps the most important—discloses our author's ingenious and most rational speculations upon the true destiny of the amyloid substance, and is, in fact, *the application*, so to speak, of the preceding chapters. Dr. M'Donnell very clearly conveys his impressions upon this point in the following sentence:—

“It is true that there is nothing novel in the view that the liver is a great blood-forming organ, or rather that it is an organ in which certain components of the blood are disintegrated, while from some of the matter so disintegrated a constant reconstruction of the blood is going forward; yet it is certain that, not long since, physiologists would have been unwilling to admit that materials constituted as the colourless blood-cells or caseine, could be formed within the liver from a substance resembling starch taking to itself nitrogen, derived, as one may say, from the retrogressive metamorphosis of tissue. It is very improbable that, looking to the liver alone, such a conclusion would have been arrived at. The consideration, however, of the physiological relations of the amyloid substance (of Bernard), as regards the development of the azotised tissues of the foetus—the fact that it is, so far as they are concerned, a protoplasma, which, by taking to itself nitrogen, terminates in the evolution of fully-formed nitrogenous tissue—prepares one to consider the idea that the liver evolves its proteic compounds during adult life by a somewhat similar process. To M. Charles Rouget we unquestionably owe the observation of the fundamental facts which lead to the foregoing conclusions; yet the author hopes that the recapitulation of facts in this memoir, will be found worthy of the consideration of physiologists; for he conceives that, not only is the view of the subject which he has ventured to adopt in harmony with a great number of hitherto unexplained circumstances, but that it gives a solution more satisfactory than any yet given of a number of pathological phenomena.”

We regret that want of space forbids our following up, at greater length, our author's excellent observations, or reproducing the description of the ingenious device by which he has succeeded in abstracting blood from the right heart and ascending cava in the living animal. We must refer those interested in the subject to the memoir itself, which we can guarantee will amply repay their study.

The subject is one of great importance, both physiological and practical, bearing as it does upon the relation of the liver to the blood and tissues; on the influence of diet upon the organ itself, and offering a clue to explain many hitherto unintelligible pathological phenomena.

Notwithstanding the difficulty of the subject, which includes a series of problems among the most complex and abstruse met with in the whole range of animal chemistry, it has been handled by our author with an amount of skill worthy of his reputation as a learned and accomplished physiologist. The essay, though short, is a model of its kind, full of patient, thoughtful, critical research, cautious generalization, logical precision, and unselfish candour.

Atlas of Surgical and Topographical Anatomy. By B. J. BÉRAUD.
Illustrated by 100 Plates, drawn from nature, by M. BION.
Translated by R. T. HULME, M.R.C.S., Eng. London: Baillière.
1866.

WE have received the first and second numbers of this work, which it is proposed to complete in ten monthly parts, each to contain ten plates, with descriptive text. Certainly if completed in the style of the parts before us it will be one of the most valuable and at the the same time cheapest set of anatomical plates yet published. The plates in the numbers we have received show the regional anatomy of the head and face. They are most carefully and correctly drawn, and beautifully coloured and finished. The names of the several parts exhibited are indicated by references, and the dissections have been so made that the relative position of each part is seen at a glance. The chief points of pathological or surgical interest, in reference to the several regions, are also briefly enumerated. We believe there is no one practising either medicine or surgery, no matter how complete his knowledge of anatomy may be, who would not find this a most useful and satisfactory book of reference.

PART III.

MEDICAL MISCELLANY.

Reports, Retrospects, and Scientific Intelligence.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.*

DR. M'DOWEL, President.

Visceral Cancer.—DR. HAYDEN exhibited a specimen of carcinoma of the thoracic and abdominal viscera. The patient was a man twenty-two years of age, of temperate habits, the son of a farmer in the King's County, who had enjoyed uninterrupted good health, never having had syphilis, rheumatism, or any other serious disease until July last. In the early part of that month he was employed cutting turf in a neighbouring bog, and while so employed had occasion to displace a large clamp of turf, and in the endeavour to do this, pushing it forward, he felt something give way in the region of the heart. This was followed by a sensation of scalding and of sinking, great difficulty of breathing, and vertigo. From this state he recovered in a few hours sufficiently to be able to return to his home; and at the end of three or four days he was so much restored to health that he was able to resume his work, but still was not quite well. Three months subsequent he complained, for the first time, of difficulty of swallowing. He experienced a stoppage in the lower part of the chest, below the ensiform cartilage, which applied only to solids, fluids passing without obstruction, but any solid food was stopped; it returned some distance; but on taking a sup of fluid, and making an effort, he was able to force it on. His mother became seriously ill, and in attending her he caught cold; had cough and expectoration, and now for the first time he had a sharp pain in the left side of the chest. He was sent to Phillipstown Hospital, where he was for some time, and was sent to Dublin on the 2nd of January. While in the train coming up he became suddenly jaundiced. On the third of January when he, Dr. Hayden, saw him for the first time, the

* These reports are furnished by Dr. R. W. Smith, Secretary to the Society.

entire cutaneous surface, was of a light lemon colour. The liver projected two or three inches below the costal cartilages, and was tender on pressure. The tongue was coated; the bowels constipated. The pulse was rapid (180 in the minute), but regular and equal on both sides. Respirations 36. The man was unable to lie down; he could only rest by leaning on a pillow placed on a table at the left hand side of his bed. There was difficulty in swallowing, as above described. The urine was loaded with bile. The left side of the chest was universally and absolutely dull, with a total absence of respiratory sound both before and behind, with the single exception that, near the angle of the scapula respiration was heard, of a tubular character, and here the voice was somewhat ægophonic. Vocal vibration was absent over the entire of left side, which, in girth, was half an inch in excess of the right side; the intercostal spaces on the left side were obliterated, but not protruded. On the right side percussion-resonance was normal, and respiration puerile. The heart pulsated with moderate force behind the right margin of the sternum, inferiorly. Its sounds were clear and unattended with murmur, but immediately before the first sound there was heard a remarkably grating noise, which, after careful and repeated examinations, he declared to be due to pericarditis. The heart's sounds were distinctly audible over the entire of the left side. There was no cough; no expectoration, and no enlargement of the special veins of the chest or neck, except two in the left axilla, which were distended, but not tortuous. There was no enlargement of the superficial lymphatic glands, or external tumour of any kind. The diagnosis was one of difficulty in some particulars. It was quite clear that there was effusion into the left pleural cavity, and that there was pericarditis. As regarded the liver he was somewhat puzzled, but after repeated examinations he came to the conclusion that there was acute hepatitis, and that in this way the extreme tenderness on pressure in the epigastrium, and the jaundice were to be explained. The man's condition remained scarcely altered until the 7th of January, that is five days after the day of his admission, when his pulse began to intermit, and he became exceedingly weak, partly from loss of sleep, and partly from inability to swallow nourishment. Leeches were applied over the epigastrium with some relief to the patient; repeated enemata were given, and opiates administered to procure sleep, and as nourishment, he got brandy and beef-tea with some bread crumb soaked in it; but on the 8th of January the pulse had descended to 150, while the respiration increased to 42, having previously been 36. From the 8th to the 14th his condition underwent a slight improvement; but on the 14th it was manifest the man was sinking, and now it was observed that the mouth was covered with aphthous ulcers. The breath was cold, the pulse flickering, and on the evening of that day he died.

On examining the body he found universal cancerous disease of the lungs,

of the liver, pancreas, and mesentery. From the history of the case, from the mode of commencement of the man's illness, and from the symptoms that followed, he had an idea that it was some form of lesion of the great vessels at the base of the heart, although there were no physical signs to bear out such a supposition; but of the absence of such, an explanation, as it seemed to him, might be found in the copious effusion into the left pleura. The peritoneum was thickened, but not otherwise altered. The liver was remarkably large, and on its surface were several cancerous nodules of considerable size. On making a section of the liver it will be seen that the interior of the organ was pervaded with similar masses. The gall bladder was enormously distended with dark green bile. The pancreas was converted into a mass of medullary cancer, remarkably firm, and nodulated. The diseased head of the pancreas was found to have implicated the common biliary duct, and so compressed it as to have caused a retention of bile, and hence the jaundice. From the suddenness of the occurrence of jaundice he thought he should look for a more direct obstruction of the biliary duct, and accordingly he tried to find a calculus which however, did not exist. The mesenteric glands were likewise diseased, and the glands in both the greater and lesser omentum were similarly affected. The stomach, the spleen, and the left kidney were unaffected, but the right supra-renal capsule was greatly enlarged, and converted into a hard carcinomatous mass; it was adherent to the inferior lobe of the liver, and he had no doubt was infected from that organ by contiguity. The kidney itself was perfectly healthy. On opening the chest he found both pleural cavities full of fluid. He should have remarked that on the 7th of January he detected crepitis in the neighbourhood of the right nipple. This was succeeded or replaced by friction, and at the same time dulness on percussion posteriorly on the right side. That was the only evidence that existed of effusion. Both pleural cavities were found filled with a green serum. The left pleura was enormously thickened; it was covered all over with a layer of false membrane, $\frac{1}{6}$ th of an inch in thickness, which might be peeled off with great difficulty from the surface of the lung, leaving the pleura unaltered beneath. The lung was remarkably heavy, perfectly solid, and of the volume of a partially inflated lung. On making a section he found the greater part of the organ converted into a scirrhus mass. Most of the blood-vessels, as likewise the bronchial tubes, were obliterated. Thickened bands of fibrous tissue, manifestly the hypertrophied interlobular tissue of the lung, were seen to pass from all points of the surface into the interior of the organ, branching and uniting so as to give the surface of section the appearance of being composed of a great number of distinct areolæ. Within these the pulmonary substance had become solidified. The walls of the bronchial tubes were likewise much thickened, whilst their cavities were nearly obliterated. The posterior margin of the lung was found thickened,

and firmly connected with the mediastinum. The right lung was in a much less advanced stage of scirrhus transformation. The arrangement of the bronchial tubes and the areolæ between them, were similar to those on the left side, but the vesicular structure had not been solidified. The base of this lung only was engaged, the superior lobe being crepitant and quite healthy. The pericardium contained a large quantity of fluid; it was thickened. The surface of the heart was rough and covered with lymph, the result of pericarditis. The heart was flabby, but otherwise unaltered, the valves unaffected. The aorta and the great vessels connected with the base of the heart were perfectly healthy.

This was a case of considerable interest, at least negatively. They had got here the most formidable disease of the lung, of the pericardium, of the liver, of the pancreas, mesentery, and of the omenta; and yet there was no direct evidence by which one could have arrived at a diagnosis of this state of things. The man was only twenty-two years of age. His illness commenced with a violent muscular effort, followed by a sensation of injury in the region of the heart, and prolonged syncope, and then pain in the left side. At no time while under his (Dr. Hayden's) observation was there cough or expectoration of any kind. There was no hemoptysis, and no enlargement of the superficial veins, except what he had mentioned, and no external tumour. The dulness on the left side was explained by the effusion, and the heart symptoms and signs by the pericarditis. The only thing which should have awakened his suspicions was the suddenness of the accession of jaundice. The tenderness of the liver was so great as to effectually prevent a careful examination of that organ; and the enlargement of the liver, and its extension downward, prevented an examination of the more deeply seated organs of the abdomen. Whether reference be had, then, to the man's previous good health, the commencement of his illness with a sensation of injury within the chest, resulting from a severe muscular effort, the short duration of his illness, or the absence of evidence of disease of the lungs, and of *all* the symptoms and signs of carcinoma, with the single exception of acute jaundice, the error of omission in the diagnosis will probably be deemed excusable.—*January 20th, 1866.*

Chronic Laryngitis.—DR. M'DONNELL said the history of the case which he had to bring under the notice of the society was briefly this. The patient, from whom he obtained the little body which he had in a test tube, was a man about fifty years of age, who had been under his care for two or three years, suffering from senile phthisis. About this time last year he lost his voice, and presented symptoms of laryngeal disease. He came often to his (Dr. M'Donnell's) house, and having examined him, he saw ulcerations at the back of the vocal cords. From the frequent examinations to which he was subjected, he became by degrees very

tolerant of examination with the laryngoscope. On the 17th of March, 1865, he came into his study, suffering from extreme difficulty of breathing; this was so great that if he had dropped down dead before he crossed the room he should not have been surprised. On examining him with the laryngoscope he found in the orifice of the rima-glottidis, a little body moving up and down. The patient said, that during the night he felt something in his throat, and that he thought it was a loose tooth that had dropped out; whenever he took a drink he felt something pricking him. Dr. M'Donnell succeeded, with a curved laryngeal forceps, in catching the little body and bringing it out at the first attempt. At first he did not know what it was, for it was apparently like hardened mucus. He put it into a tumbler of water, and after washing it and picking at it, he got out the little body which he now exhibited, and which he perceived was the arytenoid cartilage. They could see the articulating process; and the body, being partially ossified, retained its shape so completely as to leave not the slightest doubt of what it was. It was apparently held by one of the little ligaments at the base, and was in fact dangling loose in the rima-glottidis. He was not aware that there was any other recorded case in which this cartilage had been removed in this way. In laryngeal phthisis it was not uncommon for the cartilage to become affected and subject to a process of exfoliation. Professor Smith informed him that there was in the museum of the Richmond Hospital a preparation in which the cartilage was to be seen dangling loose. The patient was instantly relieved by the removal of this body, and he subsequently died, on the 29th July last, of the senile phthisis which had been going on for some time. Dr. M'Donnell, had frequent opportunities after the removal of the cartilage of examining the patient, and he saw the puckered and contracted place from which it had been removed. The loss of voice was very great; and although the patient could speak so as to be quite distinctly audible and readily understood, the power of modulating the voice was gone.—*January 27th, 1866.*

Necrosis of the Lower Jaw.—MR. CROLY said the specimen which he laid before the society was one that he removed that morning from a patient in the City of Dublin Hospital. He was a man about twenty-nine years of age, and had syphilis five years ago, for which he was treated with mercury (the special preparation being bichloride). He became profusely salivated; he had nodes on the tibia, and suffered from pains in the head. He returned home from America in March last, and when shaving one morning he found that his chin had a numb feeling—it felt as if it had been frostbitten. An abscess formed at the left side of lower jaw, between the angle and the symphysis; and being opened he got relief. An abscess then formed at the right side, and subsequently another formed under the chin. The disease went on, and

it was evident that necrosis had attacked the lower jaw. The man applied at the dispensary of the hospital, and requested that something should be done. He (Mr. Croly) found, on passing a probe through the sinuses, that it touched dead bone in three places, and on grasping the teeth the bone moved. The patient had lost two incisor teeth and one molar. That morning, by a free incision internally (detaching the lip from the lower jaw) he succeeded in exposing the diseased bone, which he removed with the "lion forceps."—*January 27, 1866.*

Fracture of the Skull.—DR. M'DONNELL exhibited the skull of an individual, sixty years of age, who died in Mountjoy Prison last December twelve months. The man had been under his observation on several occasions. He noticed that he had a scar on the side of the head, and having asked him about it, the account he gave was, that about ten years previously he had been with a number of persons making up hay, and fell down from the top of the hay rick, thereby sustaining the injury in question. Of the actual details he could not get a good account. The man was stunned by the fall, and was carried home. He was seen by a surgeon, but nothing appeared to have been done for him, and he was left pretty much to himself. How long he laboured under the injury he (Dr. M'Donnell) could not ascertain, nor whether there were any symptoms of compression. He only knew that when the man came under his notice he had got this scar remaining, and that he was a man of considerable intelligence and acuteness. On the 22nd of December, 1864, he died of apoplexy.

When the scalp was removed, the depression in the cranium, which they observed in the specimen before the society, was seen; and when the calvarium was taken off, it was found that there had been a depressed fracture, which had gone through, and the edges of which were still very sharp. There was no evidence of there having been any inflammation of the dura mater at this point—it was not thickened or adherent. There was an indentation corresponding to the depressed fracture in the brain-substance, so deep and so marked that he regretted not having been able to take a cast of it at the time.

Dr. M'Donnell observed that in this case there had been a wound of the scalp and a depressed fracture of the cranium; there was no evidence as to whether symptoms of compression existed or not; even assuming that no such symptoms had existed, yet many practical surgeons held that in cases of depressed fracture of the cranium, accompanied with a wound of the scalp, it was sound and good surgery to have recourse to the trephine. Such treatment is advocated on the grounds that it is the best mode of preventing the occurrence of inflammation of the brain or its membranes, and it is recommended as being, on the whole, the surest mode of avoiding the dangers of inflammatory irritation.

It is, however, a very nice question to determine whether this is indeed the best method of avoiding the dangers in question. The application of the trephine, and the removal of a portion of bone from the dura mater is, under the most favourable circumstances, full of risk, as an exciting cause of inflammation. The case now brought forward very well illustrated the advantages of non-interference as a preventive measure.—*January 27, 1866.*

Cardiac Disease—Patent Foramen Ovale.—DR. BENNETT reported the following case of cardiac disease, consequent on patent foramen ovale.

The history of the case extended over five years from the first appearance of symptoms of cardiac disease.

The subject of the case was an officer of the Royal Navy, and it is to be presumed that at the time of his entering the navy he was free from any signs of organic disease. In the year 1860, while on board ship, he suffered from a severe attack of measles, and during the attack was much exposed in being transferred to a shore hospital during bad weather; he rejoined his vessel in a month's time, and shortly after, on going aloft, he suffered from a violent attack of palpitation. From this time he suffered constantly from palpitations up to the time of his death, in the Summer of 1865. He was invalided by a medical board in July, 1860, the opinion of the board being that he was suffering from incurable heart disease. In August, 1860, he consulted Sir Dominic Corrigan, and was treated by him for cardiac inflammation. After having been a short time under Sir Dominic Corrigan's care he went to the country, and improved much in health, although the palpitations continued; at this time they were less frequent than at any other, being absent for nearly a fortnight, while he was out much in the open air during the day.

In May, 1861, he consulted Dr. Stokes, but there is no note of Dr. Stokes' diagnosis at that time; the treatment which he prescribed was a combination of sedative and tonic remedies, amongst others the use of the shower bath. Dr. Stokes' treatment was, after a time, followed by marked improvement. In the end of the year 1862 he was so far recovered as to be able to go to sea again, and passed as a lieutenant, after the usual practice of gunnery, &c. He was not, however, free from palpitations during this time. In 1862 he was transferred to the Pacific station, and in crossing the tropics his health gave way, but was again somewhat restored by rest at Vancouver's Island; at this time the palpitations were followed frequently by great gastric disturbance and vomiting. He came home invalided a second time, in 1864, and was again under Dr. Stokes' treatment, and improved. He again went to look for an appointment to a vessel, but while in London he had to give up his intention in consequence of his illness returning. While in London he consulted Dr. Watson, but there does not seem to have been any accurate diagnosis

made by him. He returned to Dublin, and was under Dr. Croker's care until his death. The only positive diagnosis which was made even at this time was that of hypertrophy of the heart; the left side of the chest contained a considerable amount of fluid, and he suffered much from hepatic enlargement. At no time during the course of the disease was there any cardiac murmur, though there was evidence of organic disease in the enlargement and palpitation. For some time before his death the treatment had been directed principally to relieve the distress caused by the enlargement of the liver and to quiet the heart's action. Some of the characters of the attacks of palpitation were peculiar, especially when taken into consideration with the facts revealed by *post mortem* examination. It was found that the palpitation was much relieved when he was laid down quite flat; he stated that they never came on while he was in bed, and he was often able to go through considerable exertion during an attack; he had bathed, rowed, and been on horse-back while suffering from an attack. He had not been at any period observed to be cyanosed either during an attack of palpitation or at any other time; in fact, he usually became pallid during the paroxysms.

During an attack of palpitation on July 3rd, 1865, the patient, then aged twenty-four years, died suddenly; on the following day the *post mortem* examination was made by Dr. Bennett, in the presence of Dr. Croker and Mr. O'Connor. The chest being opened, the left side was found to be occupied by clear serum as high up as the second rib in front; the lung was adherent over the highest point which the fluid reached by bands of lymph of very old standing; these connexions fixed the border of the base to the second rib in front and to the angle of the fifth rib behind; the lung was fixed to a line connecting these points carried round the pleural cavity; the left lung, compressed into the upper part of the chest, was solid below, but the apex contained air; the right lung and pleural cavity were free from any disease. The pericardium was universally adherent to the heart by connexions which could only be separated by dissection. In no part of the chest was there any sign of recent inflammation. The heart was of great size, and was distended with fluid blood on the right side; on being removed and freed of blood it weighed, with a small part of the pericardium and the arch of the aorta attached to it, twenty-four and a half ounces; its circumference, following the margins of the ventricles, measured nineteen and a half inches; at right angles to the axis twelve and a half inches. On opening the right auricle the cavity was seen to be greatly enlarged, and the septum of the auricles perforated in the position of the foramen ovale, by an opening six and a quarter inches in circumference; the right auriculo-ventricular opening was over eight and a half inches in circumference. The right ventricle was greatly enlarged also, and its walls were thickened; the valves of both openings were free from disease, but

evidently insufficient to close the openings, in consequence of their great increase in size: the septum of the ventricles was perfect, and the ductus arteriosus was completely closed. The left auricle was much smaller than the right, being not much more capacious than natural; its walls were somewhat thickened; the mitral orifice was of normal size, and its valves healthy and quite competent to close the opening completely; the cavity of the left ventricle was not enlarged, nor were its walls much, if at all, thicker than usual. The aorta was healthy, closed by perfectly healthy valves, but its size was much below that of an adult's aorta; it admitted the index finger with difficulty. The vessels springing from the arch had a calibre proportionate to the diminished size of the aorta. The liver was much above the normal size, its capsule slightly opaque, and it was congested to such a degree that blood flowed freely from the slightest scratch made with the knife; the intestines were distended with gas and congested, but there was no sign of any active disease of them.

It is probable that as there were no signs of cardiac disease in this case previous to the attack of measles in the year 1860, the following was the mode of occurrence of the pathological changes:—An attack of pericarditis occurred during the measles, perhaps brought on by the exposure in removal to hospital. With this, pleuritis, with effusion on the left side; the combination of these two impediments to the heart's action brought into play a pre-existing patent foramen ovale; then followed enlargement of the right side of the heart and contraction of the aorta. It is evident that for a long time before the fatal termination of the case the stream of blood through the foramen ovale was directed from left to right, especially during the attacks of palpitation; the character of the palpitation—namely, its being relieved by the horizontal position and its never commencing while the patient was in bed; and the pallor of the face observed during the palpitation, show that its principal cause was a want of blood in the vessels of the brain. This want of blood was caused by the deficient supply to the left ventricle, and the contraction of the aorta is the direct result of the deficient supply to the ventricle. An examination of the borders of the foramen ovale shows that the immense size of the opening was the result of the dilatation of the auricle and not from any congenital absence of the valve of the foramen. When the lower border of the opening was held up between the eye and the window muscular fibres were seen passing in curves from either side around the lower segment of the opening, and crossing each other midway between the sides; these fibres were arranged exactly as the fibres of the valve in a normal heart at birth are. The existence of these muscles in the border of the opening, while it proves that the opening was formed by dilatation of the margins and valve, explain, perhaps, to some extent the intermittent character of the symptoms during life: it is possible that although after death the opening was of so great size as to admit four fingers, it may

have been much diminished by the contraction of the fibres bounding its lower half, and have been reduced somewhat to the form of the oblique passage which is so very often seen in this place in perfectly healthy hearts. There exists in the museum of Trinity College a specimen of this kind through which a finger can be passed, but the form of the heart is perfectly natural, free from any disease, and the individual from whom it was taken died at an advanced age of a disease unconnected with the congenital opening. The fact that the foramen ovale is frequently imperfectly closed is noticed by Morgagni, who gives a drawing of the condition commonly seen. The tissue of the heart shows, as might be expected in a case where it was so much enlarged, some amount of fatty degeneration. The case is one of great interest, as showing how imperfect are our powers of diagnosis of cardiac disease, and for the late period of life at which it is possible for congenital affections to give rise to fatal disease when brought into play by some accidental occurrence, such as the attack of measles in this case. When we consider that there was not in this case any symptom present which would indicate positively patent foramen ovale, and that the cardiac symptoms did not set in until so late a period of life, it is hardly to be wondered at that even the most eminent authorities failed to recognize the lesion. The character of the palpitations are worthy of notice, though somewhat similar occur in fatty disease; still it is not common to see the horizontal position prevent their occurrence, or relieve them in the more common disease of the valves of the heart.—*January 27, 1866.*

Cardiac Disease, Mitral Obstruction, Aortic Regurgitation.—DR. HAYDEN exhibited an example of disease of the heart, consisting of contraction of the mitral orifice, and regurgitation at the aortic orifice. The person, from whose body the morbid specimen was taken, was a woman, thirty-four years of age, a native of Liverpool. She stated, that up to the age of thirteen she had enjoyed the best of health. At that time she suffered from an attack of rheumatic fever, but from this attack she recovered without any permanent ill health. She suffered occasionally from hemorrhage from the bowels, which she attributed to hemorrhoids. Three years ago she received from her husband, whilst he was under the influence of drink, a blow with the clenched hand in the region of the liver. She was knocked down by this blow, and suffered considerable pain afterwards in the region of the abdomen; and shortly afterwards was attacked with anasarca and ascites. She was treated for this attack; her legs were punctured, and there was a considerable discharge of serum. She said, that she entirely recovered from this illness, with the single exception that her respiration remained embarrassed. While coming from Liverpool to Dublin, six weeks ago, she was greatly exposed to wet on the deck of the steamboat; and, on her arrival in Dublin, her breathing

became embarrassed, and her legs swollen. She sought admission into the Mater Misericordia Hospital, on the 26th of January, and he saw her on that day. Her condition then was as follows:—Her face was quite livid, the lips in particular; the conjunctivæ were injected with purple blood; her finger ends were livid and cold, and she had lost the use of her lower extremities, which were cold, purple in colour, and enormously distended with serum, and in several parts of the surface of the legs minute openings had formed spontaneously, from which serum exuded. The pulse was rapid, exceedingly feeble, but quite regular; there was no visible pulsation of the arteries anywhere. She passed not more than three ounces of urine in the twenty-four hours, and this was loaded with bile. From the position in which the woman lay, and the danger of raising her into the sitting posture, owing to her extreme debility, it was impossible to examine the chest posteriorly, but it was examined in front, and up to the level of the nipple on both sides dulness was complete. This was attributed to effusion into the pleural cavities.

The heart pulsated in the usual situation; its impulse was pretty strong, and attended with fremitus. The heart's action, in the situation of the apex, was attended with two murmurs. One was rough, loud, and entirely confined to the region of the apex. This murmur *preceded* the first sound of the heart by an appreciable interval, and was an excellent example of what had been termed, a presystolic murmur; it was succeeded by the first sound, which was clear and unattended with murmur.

Another murmur was heard in the same situation, but less distinctly. This murmur coincided with the second sound of the heart. Over the base of the heart a single murmur only was heard; this was diastolic, coinciding with, or rather superseding the second sound; it was loudest at the base, but traceable in the course of the aorta for a distance of two inches. The liver projected considerably below the costal cartilages. It was firm on pressure, but smooth on the surface; and there was considerable effusion of serum into the peritoneum; the patient's fingers were distorted and disfigured, apparently by chronic gout. Such were the conditions on which a diagnosis of contraction of the mitral orifice was made, of regurgitation at the aortic orifice, of disease of the liver, not simply enlargement, but something more; and judging from the habits of the patient, which were intemperate, the diagnosis was made of cirrhosis. The condition of the patient admitted of no hope; all that could be done was to alleviate her sufferings while she lived. On the following day it was found that the patient had passed a restless night, having had no sleep whatever; she had now become pulseless. The punctures, which had been made on the previous day, in the legs, had yielded very little serum, and now it was observed that there appeared on the dorsum of each foot a large patch of incipient gangrene. On the

following morning, at half past one o'clock, the patient died, having for some hours before death coughed up large quantities of dark blood ; and the *post mortem* examination, made thirteen hours after death, revealed the following conditions :—

In the abdomen was found a considerable effusion of serum, but no evidence of peritonitis. The liver was contracted in its vertical dimension, that is between the thin and thick margins. It was not contracted transversely, but very much thickened. Upon the upper surface of the liver the capsules, both serous and fibrous, had become thickened and opaque. On section, the liver presented what he thought might be regarded as a good example of a nutmeg liver. The organ was otherwise not much altered ; the spleen was found in a remarkable condition—not altered in size—but the outer surface of its capsule had become thick and opaque in colour. The examination of the thick part, under the microscope, showed it to be composed of fibroid structure, or elongated fibre-cells. In the substance of the spleen two large masses of a yellow mortar or putty-like substance were found. He had examined a portion of these under the microscope and found them to consist of fat, and probably of urate of soda. He was getting a chemical examination made, and would subsequently state the result. It would not be a little interesting if they found a deposit of lithate of soda, considering that the patient's joints were disfigured as if she had suffered from chronic gout. The kidneys were healthy, the cortex, perhaps, a little larger than usual. In the pleuræ he found a considerable effusion of serum. The lungs were evidently healthy in all parts save the bases ; the bases of both lungs were opaque on the surface, and this opacity was manifestly due to thickening of the pleural and probably of the subserous fibrous investment of the lung. In this situation the lungs were dull, elsewhere resonant. This portion, on section, was semi-solid, yielding neither blood nor serous fluid. The condition of the bases of both lungs resembled that of the spleen and liver, and was due to the thickening, probably, of the fibrous investment and interlobular tissue. The pericardium contained a pint of straw coloured serum, and floating in it loosely was a flake of white membrane, attached neither to the surface of the pericardium nor to the heart. The heart looked as if daubed over with white paint ; it was white and polished on the surface, and of average size.

In the right auricle was found a large thrombus, which extended to the auriculo-ventricular opening, through which it projected by a round bulbous extremity, but did not enter the right ventricle. The right ventricle was scarcely altered, but the auriculo-ventricular opening was closed by a bicuspid instead of a tricuspid valve. The left auricle was much thickened—in the sinus the wall was exactly a quarter of an inch thick. The auricle was likewise greatly enlarged. The left ventricle of

the heart was rather contracted in size; its walls were slightly thicker; in the thicker portion they exceeded by a minute fraction half an inch. Left auriculo-ventricular opening greatly contracted, like a button-hole slit, and scarcely admitting the tip of the little finger; the mitral valve greatly thickened, rigid, and nearly calcified; the segments lay against one another, so that they must have opposed the entrance of blood from the auricle, without permitting regurgitation from the ventricle. The aortic valves were thick and rigid, and allowed the slow entrance of water into the ventricle, when poured into the aorta; the lining membrane of the ascending aorta was red, and dotted with yellow patches of atheroma in an early stage.

The only point of interest which the case possessed was that which had reference to the stethoscopic evidence on which the diagnosis of mitral contraction was made. This diagnosis was correctly made, and the evidence consisted in the existence of a murmur, audible over the apex of the heart, and confined thereto; this murmur preceding the first sound, which was clear and itself unattended with murmur. The differential diagnosis of mitral contraction was a matter of some practical importance, he need scarcely remind the society, because it was a much more formidable disease than mitral regurgitation. Thus, it was not unusually associated with gangrene of the feet, with hæmoptysis and pulmonary apoplexy, and also with total failure of the pulse some hours, and even some days, before death. Some of the highest authorities in cardiac pathology have doubts as to the possibility of accurately diagnosing contraction of the mitral orifice from stethoscopic evidence. With great diffidence he ventured to differ with them in thinking it was possible to distinguish mitral contraction from mitral regurgitation by reference to the position in time of the mitral murmur with relation to the first sound. If the murmur *preceded* the first sound of the heart, being a mitral murmur, the disease was contraction, if it accompanied or immediately succeeded the first sound, it was regurgitation. Dr. Gull reported a case in a late number of the *Medical Times and Gazette*, and whilst admitting the failure of presystolic murmur, he said the absence of it did not prove that mitral contraction was not present. No doubt the presystolic murmur ceased to be audible some time before the patient's death, and therefore his statement was correct so far, nor was it difficult to understand why this should be. A valvular murmur required for its production two conditions—one, that there should be obstruction or inadequacy at the orifice itself; and secondly, that there should be a certain amount of propulsive force, determining the current of flux or of reflux. Some time before death the force of the heart's action became diminished, and thus the removal of one of the two conditions would involve, if necessary, a cessation of the murmur.

Measurements of heart.—Right auricle, normal; right ventricle, walls, $\frac{1}{4}$ of an inch thick at apex, $\frac{3}{8}$ of an inch thick at middle; left auricle, $2\frac{1}{4}$ inches from septum to outer wall, $2\frac{3}{4}$ inches from superior wall to mitral valve, 3 inches from anterior to posterior wall; left auricle, walls, $\frac{1}{4}$ inch thick in sinus; left ventricle, cavity, from mitral valve (root) to apex, $2\frac{1}{2}$ inches, from anterior to posterior wall, $1\frac{1}{4}$ inch; left ventricle, walls, $\frac{1}{16}$ inch thick at apex, $\frac{1}{2}$ inch in middle, $\frac{5}{8}$ inch at base; aorta (ascending), 1 inch in diameter.—*February 3, 1866.*

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.
TWENTY-EIGHTH ANNUAL SESSION.

DR. M'CLINTOCK, President.

Case of Inversion of Uterus after Delivery. By Dr. DENHAM.—Out of one hundred thousand deliveries that took place in the Dublin Lying in Hospital since its foundation, we have only a single instance of acute inversion of the uterus recorded. This solitary case occurred during the mastership of Dr. Shekleton, and is given in the valuable report of the hospital published by Drs. Johnston and Sinclair.

The subject was nineteen years of age, thin and delicate. She was delivered of her first child after an easy labour of six hours. Some slight pressure having been used by the attendant, the uterus was found suddenly to recede from the grasp, and was immediately expelled from the vagina an inverted mass, with the placenta still attached to it. The patient became pallid, almost pulseless, and exceedingly anxious; complained of considerable pain and a sense of sinking.

The placenta was easily separated without hemorrhage, and the uterus returned with but little difficulty in about seven minutes. No bad symptoms followed, and she was discharged in a short time, quite well.

There are cases on record where successful attempts have been made to reduce the inverted uterus after days, weeks, and years had elapsed.

Dr. Tyler Smith replaced a partially inverted uterus of nearly eleven years standing. He passed his hand, night and morning, into the vagina for several days, and endeavoured, by squeezing and moulding the uterus with the fingers, for about ten minutes at a time, to press the tumour upwards. After repeated trials he found the cervix uteri yield and the tumour could be sunk slightly in the os. On each occasion, after removing the hand, he passed one of M. Gariel's large air pessaries into the vagina, and inflated it to as great an extent as the patient could

* These Reports are supplied by Dr. Geo. H. Kidd, Secretary to the Society.

bear. The air pessary was worn day and night, with few exceptions. From the time it was first used the hemorrhage ceased entirely, and the tumour became somewhat less in size. After more than a week of these proceedings the patient felt a good deal of pain through the whole of one night, and in the morning, when the examination was made, it was discovered that complete reversion had taken place.

Mr. Teale has succeeded by similar means in a case of two-and-a-half years' standing. Dr. Marion Sims in his *Clinical Notes on Uterine Surgery*, relates two cases of chronic inversion—one of nine months' standing, in which he failed, after repeated attempts, to reduce the inversion, and was then obliged to remove the organ with the *écraseur*; and in the other, of twelve months' standing, he succeeded almost immediately by compression with the hand.

Without further preface I beg leave to bring the following case under the notice of the society:—Jane Savage, aged twenty-three, admitted into chronic ward 12th January, states that she was delivered of her first child five weeks before admission. The membranes ruptured, and the waters kept dribbling away for two days before labour set in; on the third day there was a red discharge, and labour pains came on that evening. She was weak and faint during the night, and was delivered at seven o'clock the following morning, with only three or four expulsive pains. The placenta was forcibly extracted by the midwife in about ten minutes after the birth of the child, both by traction on the cord and pressure on the fundus of the uterus. Some difficulty or delay seems to have been experienced in its removal, for the patient described the nurse as having twisted something like a cord round the wrist of one of her hands, with which she made considerable traction, while at the same time she strongly pressed on the belly with the other. During this time she suffered a great deal of pain, but suddenly got relief by the expulsion of a large tumour from the passage, which led her to exclaim—“Oh, Mother of God! am I going to have another!” The placenta, which was partially detached, was now entirely separated, and the uterus thrust into the vagina. The poor patient remained weak and exhausted all the day, passing from one attack of syncope to another, until four o'clock, when she was seen by the dispensary doctor, who, unfortunately, only felt the pulse and looked at the patient, but made no examination. She remained in bed for eleven days, and then got up a little every day until her admission; the doctor saw her twice, but never made a vaginal examination; in fact, he never diagnosed the nature of the case. During all this time she had a continual shedding, especially at night.

She came into hospital in a most pitiable condition—pale, weak, and exhausted, from the combined loss of blood, appetite, and rest. In making an examination we thought we had simply to do with a

case of polypus; but on passing the finger round the neck of the tumour we could not discover the os uteri, while at the same time the tumour wanted the smooth polished surface generally met with in such cases; we therefore came to the conclusion that it must be a case of recent inversion of the uterus. A generous diet, with a liberal supply of wine, was ordered. Citrate of iron and quinine, with an anodyne at bed-time was prescribed, with perfect rest in the horizontal position. The poor woman greatly improved under this treatment, so that we were able to attempt the reduction on the fourth day after her admission. Having been brought fully under the influence of chloroform, she was placed on her back, with the thighs flexed on the pelvis, and the legs on the thighs. The hand was then slowly introduced into the vagina, and the fundus and body of the inverted uterus firmly grasped with the fingers and thumb. Steady gentle pressure in this way was brought to bear on the entire tumour for several minutes before any attempt was made at reduction. The tumour gradually diminished, partly from the pressure and partly from the loss of blood, which was very considerable. Pressing steadily upwards, the uterus was now felt gradually to yield, and in a short time the fundus alone remained unreduced; no amount of force, however, compatible with the safety of the organ could enable us to complete the operation, and as the patient was faint from the loss of blood, we did not consider that we were justified in making any further attempt at reduction for the present. The vagina was now syringed out with cold water, the patient was replaced in bed, and a full anodyne administered. On the following day we found her with a rapid pulse and complaining of pain and tenderness over the uterus. Opium, with small doses of mercury, were freely administered, and linseed poultices were kept over the abdomen constantly.

The symptoms gradually subsided; and on making a vaginal examination on the third morning after the operation we were agreeably surprised to find that the fundus had spontaneously returned either by its own elasticity or the contraction of its muscular fibres. For many days we had a very profuse purulent discharge from the uterus, but the patient steadily improved in appearance and health, and was able to get up for a few hours every day at the end of a fortnight. On examining with the speculum about a week after the reduction we found the os uteri ragged and inflamed, but the sound passed up readily into the cavity of the uterus without causing much pain or uneasiness. She was discharged from hospital in perfect health, having menstruated regularly a few days before leaving. We have nothing further to add, only to express our regret that the accident was not discovered immediately on its occurrence. It is useless to speculate on the result had the fundus not gone up of its own accord after the first attempt at reduction; it is possible that a

repetition of the former plan of treatment might have succeeded. The successful plan followed by Dr. Tyler Smyth, with M. Gariel's air pessary, might have again been tried, but I am free to confess that the careful daily manipulation of squeezing and moulding the displaced uterus had more to do in the reduction than the air pessary, as I can scarcely believe that it could be distended sufficiently to act on the constricted portion of the uterus, which, in my case, at least, appeared to be at the upper part of the unreduced organ rather than in the portion near the vagina; but this, of course, is mere matter of conjecture.—10th March, 1866.

DR. WILSON read the following paper:—

Ophthalmia of New-born Children.—Purulent Ophthalmia of Infants; Ophthalmia Neonatorum; Purulent Eye of New-born Children (Ware, 1805). Blepharophthalmia Glandulosa Neonatorum (Benedict, Beer). Blepharitis Puriformis Neonatorum; Lippitudo Neonatorum; Blepharoblennorrhea Neonatorum; Blepharophthalmia Blennorrhoeica Recens Natorum. Ophthalmoblenorrhoea Infantum (Stellwag).

The earliest observation respecting this affection, with which I am acquainted, is that by John Peter Albrecht "De Lachrymis Lacteis Fluxuque Oculorum Chyloso in Infante Recens Nato," contained in the ninth volume of the *Miscellanea Curiosa*, published at Nuremberg, 1691.

In 1750 Samuel Theodore Quelmalz, also a German, published his *programma de caecitate infantum, fluoris albi materni ejusque virulenti pedissequa*.

The first accurate account of the disease was written in 1780 by James Ware, an English surgeon of great merit.

Independent of Ware, and, as it would appear, totally unacquainted with his essay, William Dease, one of the most distinguished men of his day, and who by his teaching, and learned as well as practical writings, contributed in no small degree to the fame and celebrity of the Dublin School of Medicine, published, in 1783, in his *Observations in Midwifery*, a description of this malady.

In 1824 Isaac Ryall, "Surgeon Oculist to the National Infirmary for Curing Diseases of the Eye in Dublin," published a very good account of the disease in the fourth volume of the *Transactions of the Association of the Fellows and Licentiates of the College of Physicians*.

In 1835 Dr. Evory Kennedy read a valuable memoir before the British Association, then holding its meeting in Dublin.

In 1839 Hugh Carmichael wrote a long and faithful account of the disease in 15th vol. of the *Dublin Journal*. Many of those present may recollect how energetically he advocated the administration of grey powder.

Evanson and Maunsell mention it very briefly in their *Management of Diseases of Children*, 1838.

Drs. Johnston and Sinclair give a short chapter on the subject in their *Practical Midwifery*.

Sir Wm. Wilde alludes to it in his "Reports on the Progress of Ophthalmic Surgery," *Dublin Quarterly Journal*, 1847 and 1848.

Drs. M'Clintock and Hardy have written a very valuable and practical chapter on ophthalmia neonatorum in their *Observations on Midwifery*, 1848.

Purulent ophthalmia may occur at any age; it is frequently seen in early life, and but rarely in advanced age; it is sometimes exceedingly rapid in its course, and generally most destructive to the eye if neglected. No matter at what age it occurs, however, whether in the young or the old, the disease is in all respects alike; its symptoms, progress, and results will be almost the same. The remarks I am about to make will, therefore, be on the whole equally applicable to the affection in new-born children or in youths and in adults.

Ophthalmia neonatorum is an inflammation of the conjunctiva with mucous or purulent secretion, and manifests itself on an average on the third day—nearly invariably within a week after birth; it may occur a few hours after birth, or not appear for a fortnight subsequently; its severity would appear to be in proportion to its early occurrence. We rarely witness it in its earliest stage, owing to the ignorance or neglect of the mother or attendant. The lids, at the commencement of the disease, are somewhat red at their margins; the conjunctiva lining the lids slightly red and vascular; a small quantity of whitish mucus is usually found at the inner angle or lying in the fold between the globe and lower eyelid. There is slight intolerance of light, and the eyelids are kept closed, and after sleep, are found stuck together. The globe of the eye appears quite normal. Advice is very rarely sought for at this early period—the infant is incapable of expressing its particular sensations or sufferings; the redness, even if perceived by the mother or nurse, is attributed to a mere cold, and the eye is bathed in some warm fomentation, or mother's milk, &c.

After a couple of days the intolerance of light increases, the lids begin to swell, and a discharge occurs; the infant is irritable and fretful. If the disease progresses the lids become more and more swollen and red, especially the upper one, which not unfrequently overlaps the lower one; the eyelashes are matted together by the discharge, which becomes hardened and gums the eyelids together. On opening the eyelids, the discharge, which is generally of a pale yellow colour and rather thick consistence, flows out over the cheek. The conjunctiva of the lids and globe is intensely red and vascular, and generally protruding slightly beyond the level of the cornea.

Should the disease continue unchecked the eyelids become more and more swollen and prominent, partly from serous effusion into their

texture, and partly from the accumulation of secretion between them and the globe; they are shining, extremely red or purple, with distended and engorged veins, and present a truly alarming and terrifying appearance. One or both of the lids may be found everted. The infant is suffering pain, as is evident as well from its irritable cry as from the wrinkling of its forehead and corrugation of its brows. In this state it is usually brought for advice and treatment. On opening the lids a gush of yellowish matter occurs, and the upper lid frequently turns inside out; bleeding also occurs frequently. The conjunctiva has been resembled to the injected fetal stomach: it bulges forwards and overlaps the cornea constituting what is termed chemosis. From the great swelling of the ocular and palpebral conjunctiva, and also owing to the facility with which the eyelids become everted, it is very difficult to get a view of the cornea. That membrane is at this stage of the disease generally greyish, either in its entire extent or at one or more spots: it has lost its polish, and the whole of its external epithelial layer may present a macerated appearance; its laminae may be infiltrated with pus, or it may present an ulcerated condition. This ulcer is, as a rule, circular and near the centre; it may, however, be at the periphery, and horse-shoe shaped, when it will be more dangerous to vision; the anterior chamber may also contain pus. This condition may exist for several days, the lids appearing almost on the point of bursting, and the discharge becoming more and more profuse, until finally the cornea gives way, and allows the escape of the aqueous humour. The tension of the globe being removed relief quickly ensues; the tumefaction and redness of the lids begin to subside, the swelling of the conjunctiva diminishes, and the discharge becomes thin and greenish; and when we obtain a view of the parts we find the cornea perforated and the iris protruding.

The character of the discharge varies: in some it is whitish and simply mucous; flakes or shreds of mucus are seen floating or lying in the abundant lachrymal secretion. In such cases the disease is, as a rule, mild, and partakes more of a catarrhal nature. In other instances the discharge is yellowish or greenish, and essentially purulent, always coexistent with a very high degree of inflammation, and great danger to the eye.

The disease commences frequently only in one eye, and subsequently attacks the second.

The duration of the disease is about three weeks; even in favourable cases it rarely terminates completely under a week. In mild forms the disease may continue for weeks, and finally terminate favourably. In very severe cases vision, or even the globe, may be destroyed in a very few days; in this, as well as other respects, the affection bears a very close resemblance to leucorrhœal or gonorrhœal ophthalmia in the adult;

it has indeed been denominated "ocular gonorrhea of the new-born" (Augentripper) by a German writer (Eisenmann).

It is nearly always a matter of difficulty to obtain a view of the cornea and iris. The lids are enormously swollen. On attempting to retract them from off the globe they very easily become everted, the conjunctiva is distended, and tumefied, the globe is rolled upwards, and all the parts covered with matter. All these circumstances, added to the infant's unsteadiness, render it difficult to get a good or continued view of the cornea. It may not be out of place, therefore, to offer a few remarks on the mode of examination. The nurse should be put sitting in a chair opposite a window, the infant placed on her lap also facing the light, with its head resting against the lower part of the nurse's chest, and its feet or clothes secured between her knees. The nurse's arms should be placed around the infant's body, securing its arms and hands. The examiner, standing or seated to the right of the nurse, then places the flat of his left hand on the top of the infant's head, spreading out the four fingers fan-shape, so as to embrace as much as possible of the head—bearing in mind that the anterior fontanelle is still open—and brings down the thumb of this hand over the forehead so as to elevate either the right or left upper lid, as may be desired. The right hand is then placed under the chin, or angle of jaw, with the fingers spread out, so as to embrace the lower jaw on each side, while the thumb is at liberty to draw down the lower lid; or this hand may be used to cleanse the eye. By this means the child's body and head are steadied and fixed; and even if the head does move the examiner's hands must participate in the movements, and his fingers cannot do any mischief to the eye. I would especially direct attention to the great danger incurred in separating the eyelids by means of the points of the two index fingers; in any sudden motions of the head forwards or latterly, the globe may be pushed against the fingers, and thus subjected to violent concussion. If the cornea be ulcerated or perforated, the most disastrous results may ensue. It has happened before now—not a little to the horror and consternation of all concerned—that the whole contents of the globe—aqueous humour, crsytalline lens, and vitreous humour have been shot out on the examiner's hand, or on the child's face. In order to avoid eversion we should, in raising the upper lid, seek to raise it bodily in a straight line towards the eyebrow, keeping it gently pressed against the globe. I am in the habit of placing the nail of my thumb or index finger under the free margin of the lid, and then steadily and slowly pushing or raising up the lid. This requires, however, some dexterity and practice, and is a manœuvre I could not recommend to those who are not very conversant with manipulations about the eye. We may occasionally get a glimpse of the cornea by seizing the eyelashes of the upper lid between finger and thumb, and drawing the lid away from the globe. Should these methods

fail, and it be considered necessary to obtain a view of the parts, recourse must be had to elevators or retractors. I consider those of Desmarres—now before you—the best; one for the upper and one for the lower lid; they are smooth, polished, and curved, so as to be adapted to the inside of the lids. Care must be taken not to exert any pressure on the globe; and it will be necessary for an assistant to hold the head, or for the examiner to take the child's head between his knees. The lower lid is so easily managed I need not dwell on it. We should be always careful to replace an everted lid, or it may otherwise remain everted for some days, as the conjunctiva becomes swollen and enlarged when exposed. I have dwelt thus long and, perhaps, tediously on these matters; as I consider it of great importance to be able to examine an eye with safety to the organ and satisfaction to ourselves; moreover, we should never give an opinion as to the result without having first satisfied ourselves of the state of the cornea, iris, &c.

Various *causes* are assigned for this disease, such as heat, cold, draughts, sudden changes of temperature, bright light, irritating substances, such as soap, whiskey, gin; and, finally, vaginal discharges. I do not believe however, that either light or heat, such as the infant's eyes are exposed to, can produce an inflammatory condition of the conjunctiva. The infant's eye is, I think, comparatively insensitive to light. It has also been proved by experiment that heat affects the retina but very slightly. In the adult we daily witness affections of the nervous apparatus of the eye as well as of the nervous centres, and conjunctivitis is an exceptional complication. Exposure to cold draughts, sudden and marked changes of temperature, vitiated atmospheres, may and do, I believe, contribute largely to the production of this disease; but in such the affection, as a rule, manifests itself late, the symptoms are mild, the discharge mucous, and recovery generally takes place without any very bad results. The cause which I regard as the most frequent, as well as the most dangerous, is the direct application to the infant's conjunctiva of either gonorrheal or leucorrheal matter, or of morbid secretion from the eyes of other individuals. The members of this society are well aware of the very great frequency of chronic vaginitis or leucorrhea in pregnant females. Now, it is readily conceivable how some of the vaginal discharge may gain access to the child's eye during birth, and set up an inflammation of a similar nature, unless speedily and effectually removed. It may be urged by some that the child's eyes are closed during birth, and otherwise protected; but the eyes are opened on the head emerging, and there is plenty of opportunity for their receiving the virus before the birth is completed, either directly or by means of the deliverer's hands. Another objection has been advanced, viz., that if the conjunctiva be inoculated, so ought also the mucous membrane of the genitals of the female; but it appears to me these parts are to a certain

extent naturally protected from contact with the vaginal walls, and they are, moreover, I think, much more effectually cleansed by immersion in water after birth. The highly contagious nature of leucorrhea is admitted by all, and my only wonder is how so many infants of infected mothers escape. Of 328 women who were delivered in the Lying-in Hospital in Stockholm in 1832, 181 were free from vaginal discharge, and 137 affected therewith; 80 of the children had ophthalmia. Of these 20 belonged to infected mothers and 10 to non-infected. While these figures prove that the disease does not in all cases arise from this cause, they show that it does so in the majority of cases, the proportion having been 1 in 7 of affected mothers, and but 1 in 18 of non-infected mothers. These statistics agree with the opinion expressed by Drs. Hardy and M'Clintock, who state that while the disease "was not exclusively confined to the infants of women suffering under leucorrheal discharges or gonorrhea, although where such existed the child was more liable to the disease."

Opinions are still divided in this matter; my own experience is that in the great majority of instances the mother is the subject of vaginal discharge. Dr. Churchill, however, in his valuable work on *Diseases of Women*, states that he has never met with a case of purulent ophthalmia caused by the infant's eyes coming in contact with the leucorrheal discharge during the passage of the head through the vagina.

The disease is also occasionally communicated by means of sponges, towels, and other articles which have been in contact with morbid infectious secretion elsewhere.

It is occasionally epidemic; and in some places, such as foundling hospitals, workhouses, maternities, &c., endemic. It is highly contagious, for we sometimes see melancholy cases where the mother or attendant has been seized with the same disorder.

The mildness or severity of the attack will depend principally on the cause: it is most formidable when caused by gonorrhea or leucorrhea. The disease may terminate favourably, and leave no trace of it whatever; a very common result, however, is opacity of the cornea, which may occupy a greater or less extent, or involve all the layers, or be simply superficial. The iris may be adherent to the cornea, forming synechia anterior, which results from ulceration and perforation of the cornea, with prolapse of the iris, and in consequence thereof the pupil may be distorted in various directions, or be completely obliterated; or there may result a small white speck on the front of the capsule of the crystalline lens—denominated central cataract, and often, erroneously, congenital cataract. This arises when (the cornea being burst and the aqueous lost) the capsule comes in contact with the ulcer, and lymph adheres to it. The crystalline lens may also escape, and so also may the

entire contents of the globe, and the eye collapse; or the globe may become staphylomatous and project between the lids, presenting a most unsightly object, as well as being a source of great danger to the other eye.

Terrible and alarming as the disease undoubtedly is, it is satisfactory to know that it is perfectly amenable to and controllable by treatment. The principal part of the treatment consists in constant and effectual cleansings and ablutions. For this purpose tow is preferable to sponges, as the latter are liable to be used for other purposes, or by other individuals, and so to propagate the disease; the tow should be always thrown away after use. The lids should be well fomented with warm water, so as to soften the discharge which has collected on and matted together the ciliæ. The lids are then to be drawn asunder, and the discharge *effectually* wiped away from off the conjunctiva. A stream of water pressed out of the tow may be allowed to fall from some little height into the palpebral aperture. The water for this purpose may be very nearly cold. This extemporized douche will in many cases be found most serviceable, not alone in the ophthalmia of infants but also in juveniles and adults. I have seen it carried out with beneficial effects in a large number of cases by my friend M. Chassagnac, of Paris. Sir Wm. Wilde, many years ago, had a special apparatus fixed up in St. Mark's Hospital for this purpose. I order the affected eye to be cleansed every hour, and generally apply a solution of nitrate of silver (2 grs. to the oz.) every morning or every second morning, and prescribe a lotion of some astringent substance to be kept constantly applied to the eye. I would prohibit altogether the use of acetate of lead, which was first recommended in these cases by Dease, for if there be any ulceration it is likely to leave a white deposit on the cornea. The lotion should be applied by means of a single fold of fine linen or lint, which should be constantly changed or remoistened in the lotion—the effect of several folds will be that of a steam-bath or poultice, and this I would avoid. The infant's health should, at the same time, be attended to; its bowels freed by castor oil, or grey powder, with aromatic powder, or grey powder and rhubarb, &c.; and all causes likely to keep up or aggravate this disease removed. Changing the infant to another apartment will sometimes be found most serviceable. In the milder forms of disease, or when it is seen early, this treatment will suffice; but the majority of cases are not brought to us until there is great chemosis and the cornea is threatened with destruction. Here I clip the chemosis with a curved scissors, and scarify the conjunctiva of lids by lightly running the convex edge of a small scalpel across it in different directions. By this means we not only deplete and relieve the great congestion, but we also remove the pressure from off the globe, and more particularly from around the cornea, by which its vitality is threatened. The bleeding may be

encouraged by warm fomentations. My objection to leeching infants is the very troublesome and uncontrollable hemorrhage which sometimes ensues, as well as the unsightly marks which occasionally result. In severe and rapid cases the chemosis may be clipped a second or third time in the day. The affected eye should be washed at least once an hour. I apply a strong solution of nitrate of silver, 5 or 10 grains to the ounce, to the conjunctiva, after effectual removal of the secretion, and prescribe astringent lotions. I have in a very few cases employed the *lapis mitigatus*, but I prefer the solution of nitrate of silver. *Lapis mitigatus* consists of equal parts of nitrate of silver and nitrate of potash fused, and run into moulds.* After its application the residue should be washed off with solution of chloride of sodium. I always use in these cases a solution of sulphate of atropia. The atropine solution of the *British Pharmacopæia* is objectionable in ophthalmic practice from the large quantity of spirit contained therein, which is very painful and irritating. I now always employ the sulphate of atropia, which is readily soluble in distilled water. The object of this preparation is to keep the pupil dilated, whereby the iris is prevented to a certain extent from prolapsing through the cornea in case of a perforating ulcer, or even if recently protruded the atropia will retract the iris. I am inclined to think that dilatation of the pupil diminishes intraocular pressure and relieves vascularity, and therefore use it largely in all ocular inflammations.

The bowels are to be freed ; and I prescribe tonics, frequently syrup of bark, to which may be added, if the infant is very restless and irritable, tincture of hyoscyamus. It may be necessary also to attend to the mother's or nurse's state of health, and occasionally a warm bath will be useful to the infant.

Should the disease be endemic, stringent measures must be adopted to eradicate it.

The mother or attendant is, in every case that comes before me, warned of the contagious and dangerous nature of the malady ; and I am in the habit of advising the mothers to place themselves under special treatment in their next pregnancy, with a view to the cure of the vaginitis.

A consideration of the results of this disease and their treatment would be scarcely suitable to this society ; it would, moreover, require one or more separate papers. I may mention, however, that the opacities of the cornea, if uncomplicated with adherent iris, disappear almost entirely. I have been astonished at the rapidity and completeness with which they sometimes disappear. Not long ago a medical friend placed under my care an infant with the disease in both eyes. The right cornea

* This preparation may be had at Bewley and Hamilton's, Sackville-street.

was completely opaque, and the iris protruding: the left cornea nearly entirely opaque. The case was so bad I gave an opinion that the child would have but limited vision, and that on the left side only. Notwithstanding, however, I kept atropia solution constantly employed, &c., and the child now, after four months, has almost perfect vision in both eyes, the prolapsed iris having returned to its normal position.—*April 14, 1866.*

A Case of Double Monstrosity. By Dr. M'CLINTOCK.—The specimen now exhibited to the society, was sent to me by Surgeon Fitzpatrick, of Enniskillen, who has since presented it to the Museum of the Royal College of Surgeons. Dr. John Barker, Curator of the Museum, has favoured me with a brief anatomical description of the monster, so far as he has been able to dissect it, without injury to its shape and external configuration.

"The specimen," writes Dr. B., "may be said to consist of two female children, joined together by the anterior part of each thorax in a nearly symmetrical manner, from about one inch below the chin of each to the umbilicus, which is single. The shoulders, on one side, are about three and a half inches apart, and on the other side about two and a half inches. The limbs are perfect, and well formed, and there is only an arrest of development of one external ear. On removing the integuments, in the median line, between the two, from above downwards, the thymus gland of each side was distinct. Then, from above downwards, we see the large radiating fibres of the great pectoral muscle, larger, I think, than in the normal state; the left was larger than that on the right side. A little below the episternal notch, on the right side, a band of muscular fibres extended downwards, and outwards on the surface of the pectoral, and were lost on the rectus abdominalis. On cutting at each side of the common sternum, and through the clavicles, and throwing down this flap, the following parts were exposed: on the right, what appeared to be a pleural cavity; on the left the pericardium, which rested on the diaphragm below, as in the normal state; no lung was seen.

"On opening the pleural cavity the whole of the right side of the thorax was occupied by the liver, which extended downwards, and to the left side, joining that of the second fetus under the diaphragm, which was found to be deficient in the right side of the abdomen. Upon opening the pericardium we had the heart not much larger than natural. From its base three vessels appeared to arise, one for each fetus, and one or two in the median line. There were two sets of hypogastric arteries."

One of the fetuses, I may remark, is somewhat smaller than its fellow; and, judging from their bulk and general development, I should not suppose they were more than eight months children. From the gentleman who sent up this preparation, I have learned the following particulars of

the labour. The mother was a healthy woman, aged twenty-six years, and this was her fourth confinement. When Mr. Fitzpatrick was brought to see her she had been suffering for some days with short, teasing, ineffectual pains. He was at once struck by the enormous size of the abdomen, and, concluding that the imperfect uterine action was due to over-distention of the uterus, he gave a dose of ergot; and, as soon as this had excited some pains, he ruptured the membranes, whereby about three gallons of liquor amnii were discharged. On now passing the hand over the uterus he distinctly felt two heads. As no presentation could be reached, he, with some difficulty, passed his hand into the uterine cavity, and brought down two feet. No pains coming on, he administered another dose of ergot, which acted well; nevertheless, his efforts at delivery were unavailing. To find out the cause of this difficulty he passed up his hand into the vagina, and, to his great surprise, found there the knee of another child. He tried to push it back, but could not, in consequence of a strong pain coming on. The four legs were now down, and, under the influence of powerful uterine contractions, he was able to withdraw the bodies and heads without much delay. The children were born alive, and lived for about twenty minutes. There was one large funis and a very large single placenta. The mother was quite well and up within a week afterwards.

Monsters of this kind are, happily, of extremely rare occurrence. I do not believe that the experience of the Lying-in Hospital supplies a single instance of such. Nevertheless, many are to be met with in the records of midwifery, and especially in the *Transactions of the London Obstetrical Society*. To us accoucheurs the point of greatest interest connected with these monsters is the manner of their delivery, and upon this point I wish to offer a very few observations.

Treatises upon midwifery give us very little information on this matter. Dr. Churchill (*Theory and Practice of Midwifery*, fifth edit., 491), is satisfied with quoting the observations of Dr. Burns, viz.:—"The general principle of conduct must be, that, when the impediment is very great, and does not yield to such force as can be safely excited by pulling that part which is protruded, a separation must be made, generally of that part which is protruded, and the child afterwards turned, if necessary. Unless the pelvis be greatly deformed it will be practicable to deliver, even a double child, by means of perforation of the cavities, or such separation as may be expedient, and the use of the hands, forceps, or crotchet, according to circumstances. A great degree of deformity may render the Cesarean operation necessary."

These directions, it must be confessed, embody no principle of treatment, and are altogether too general to afford us any guidance in particular cases.

Now, I believe, that in regard to delivery, the two fetuses should be

treated as a *single one* ; and, as in single births we always aim at delivery by the head or by the pelvic extremity, so, in the case under consideration, we should endeavour to bring the monster through the pelvis, by the two heads simultaneously, or by the two pelves (or pelvic extremities) simultaneously. Acting in accordance with this precept will entail less trouble, I think, than if we proceed by getting one of the fetuses disengaged from the pelvis, before the other. If the fetuses be small, as they generally are, their extraction together, will not be attended with any very extraordinary difficulty. If, on the other hand, they be full sized, or the pelvis be small, the difficulties to be overcome in their extraction, will not be so great, I conjecture, as if the other mode of delivery be followed. I am, of course, taking it for granted that the exact nature of the case is known to us. But this knowledge is seldom acquired till the delivery is too far advanced to admit of any material alteration in our plan of proceeding. The difficulty most likely to present itself is, to decide whether the second head, or second set of legs, following upon, or along with, the first presentation, belongs to a twin fetus, or appertains to a double monster ; and, yet, upon this rests an essential difference in the treatment. For, in the case of twins, the intruding part of the second fetus should be pushed up, so as to let the other fairly engage in the pelvis ; but with a double monster, the two heads, or two sets of pelvic extremities, should be encouraged to enter the pelvis together—at least if the principle I have laid down be the correct one. How, then, are we to distinguish a double monster from twins, under the above circumstances ? Whilst admitting that this is a very pertinent question, I must, at the same time, confess it is one not easily answered. The presence of two fetal hearts, at some distance apart, or of two separate sets of fetal membranes, would prove the case to be twins ; but in the absence of these signs, to establish the fact of a double monstrosity, at this period of the labour, can, perhaps, only be done by careful digital or manual exploration.

TRANSACTIONS OF THE COUNTY AND CITY OF CORK
MEDICAL AND SURGICAL SOCIETY.*

SESSION 1864-65.

PRESIDENT in the Chair.

On Digitalis, and its Uses in Medical Practice. By H. MACNAUGHTON JONES, M.D., one of the Demonstrators of Anatomy, Queen's College, Cork.

A drug so often commented on, about which, from time to time, so many discussions have arisen ; it may appear rather hackneyed to bring it

* These reports are supplied by Dr. Curtis, Secretary to the Society.

forward for further comment ; yet I think there are still some points of sufficient importance, interesting alike to seniors and juniors, that warrant a careful notice of its effects, as practice and theory have demonstrated, and experience proved.

Digitalis purpurea, better known as foxglove, appears to have been first noticed particularly by Haller and others of his time, and strange to say, then as a remedy for scrofula, for which, of course lately, it has gone out of use ; but Withering appears to have been the first who drew the particular attention of the profession to it as a remedy in dropsies, cardiac, and renal, as a sedative of the circulation, and as a diuretic.

Its well-known appearance often imparting, during the months of June and July, a brilliant look to our Irish ditch sides, with its beautifully tinted spike, covered with pendulous flowers, purple, or rarely white, contrasting well with its peculiar thick leaves, downy on their under-surface only, this latter characteristic distinguishing them from those of their adulterations, comfrey and *verbascum thapsi*, while their peculiar bitter taste also serves as an additional peculiarity.

In considering it :—

1st. I would first propose to glance at its properties dependent on its active principles.

2nd. Its various applications in disease.

3rd. To consider some of the causes of the apparent discrepancies as regards its administration.

In doing this, I must here tender my thanks to Professor O'Leary, Queen's College, Cork, for some valuable information he has afforded me on this subject—valuable especially as he has had opportunity of witnessing experiments performed by some of the first continental authorities on the drug. Before considering its active principles, and their effects, I would wish to notice, first, that there is no drug, the collection, preparation and preservation of whose leaves greater care must be bestowed on, than *digitalis*. The time of year, the age of the plant, are perhaps two of the most important points. Waltz, of Heidelberg, who has spent an immense amount of labour in investigating this matter, found that the quantity of active principle, present in various specimens, differed immensely. The exposed position of the plant—or, on the other hand, its sheltered one—the degree of moisture, all influence its activity, and consequently the activity of its preparations ; this fact is not confined to *digitalis*, it is a botanical law ; from all experiments it appears that the best time to collect the leaves is during June or July, when the plant is beginning to flower, and that the largest and deepest coloured are the best to select, removing the petiole and mid-rib, and carefully drying them, not tying them in bundles, as is sometimes done, as that is likely to cause fermentation. They should be kept from the action of light, in dark bottles, and in dry

places, and renewed yearly. I think the odour of a bottle of good digitalis more resembles that of a canister of tea than anything I know of.

Active principles.—If now there exists, as we shall presently perceive, a variety in the active principles of digitalis, it follows that unless all tend to the same action, the effects we desire to produce would be greatly altered, their actions individually not being the same, by prescribing them together or separately. Hence, we find, in Paris, Bouillaud uses the granules of digitaline in heart affections. Let us now, therefore, consider what are their active principles, and how they differ in their respective effects. The older writers appear to have looked on digitalis as diuretic, through its sedative action on the heart promoting thus absorption. Dr. Paris so looked on it, and they rightly supported their views by pathological and physiological facts of poisons being absorbed so readily in languid circulations, and digitalis not having the same diuretic effect in health as it has in diseased states; but beyond this they do not appear to have gone, and they did not nicely discriminate between the different actions of its active principles. Now, numerous substances have been asserted to be present in digitalis—the result, perhaps, rather of the manipulating ingenuity of the enthusiastic chemist. And the names of these would be but a useless category. I think, finally, we may reduce the number to three of any importance. The two following analyses I lay before you are those of Waltz, Quevenne, and Homolle:—

ANALYSIS OF HOMOLLE AND QUEVENNE.

1°. DIGITALOSE.—Shining, Snow-white; needle-shaped crystals; soluble in alcohol and chloroform; more sparingly in ether, turning yellow by 80°, and neutral in re-action.

2°. LE DIGITALIN.—Without smell or taste; freely soluble in alcohol; sparingly in chloroform; turned first grey, then brownish, by Hcl.

3°. DIGITALID.—Having a bitterish sweet taste; very sparingly soluble in alcohol, and electro-negative.

4°. DIGITALIC ACID, white; crystalline; when heated having a penetrating smell; easily decomposed on exposure to air, and turning brown; freely soluble in water and alcohol, and somewhat in ether.

5°. ANTIRRHINIC ACID.

4°. DIGITALIMIC ACID, closely resembling the Oleic.

Of these the three following are those, I think, of special importance. I mean, viz.:—digitaline, digitalic acid, and volatile oil; these having been isolated and experimented with.

WALTZ ANALYSIS.

1°. DIGITALIN ($C^{10}H^9O^4$).—Crystalline soft mass; freely soluble in alcohol, especially hot; sparingly in water and ether.

2°. DIGITALOSIN ($C^{19}H^{16}O^9$).—An amorphous, yellowish white, extremely bitter substance.

3°. DIGITALERIN ($C^{11}H^{10}O^5$).—A yellow, very acrid deliquescent powder.

4°. VOLATILE SUBSTANCE, obtained by distillation, closely resembling valerianic acid.

These four constitute what is generally known as Digitaline.

2. ACIDS: DIGITALIC & ANTIRRHINIC.
Former—fixed, soluble in water and alcohol, containing no nitrogen, and having a peculiar smell.

Latter—grey, colourless, valerianic smell

3°. PIKRIN, a bitter, deliquescent substance.

4°. SCAPTIN, a brown, acrid extract.

5°. Chlorophyl, starch, gum, lignin, tannin, salts of lime and potash, volatile and fixed oil.

Now, of these three, one appears to have an action peculiar to itself—the digitaline—a pure sedative, reducing the beats of the heart, and producing a slow and irregular pulse. In Homolle's latest experiments it was satisfactorily proved that from two to six milligrammes invariably caused a fall of from four to twelve beats a minute, in the heart's action, in the physiological state, and a diminution of one-half, attended with restored regularity of action in persons labouring under affections, organic or functional, of the heart.

Velpian experimented largely on dogs, which were found to be very easily affected by digitalis; and his views, that its action is especially manifested on the organic contractions of the heart, which at first lowers, and finally stops, have been confirmed by the researches of Gouleaud and Homolle. In France, during its administration, the beats of the heart have been lowered twenty in a minute by giving twenty drops, every third hour, of the tincture. The French granule of digitaline, perhaps preferable in cardiac disorders, containing fiftieth of a grain, commencing with one, and gradually increasing every sixth or seventh hour to five.

Of late, during the French poisoning trial, more particularly investigated by Hebert Homolle, digitaline was recognizable as poison by,—first, being turned brown by concentrated sulphuric acid; this gradually emerging into a dark-red colour, which latter is turned into a dirty green on the addition of water. Second.—When moistened with sulphuric acid, and exposed to the vapour of bromine, a beautiful violet hue. The poisonous effects were found to require for their production much larger doses when digitaline was given to dogs, than when taken by man. The most constant symptom first manifested, was a peculiar kind of vomiting subsequent on, and seemingly a consequence of, the extension upwards of convulsive spasms of the extremities. These effects were found to take place much more quickly and with much more intensity when digitaline was applied to a wound or introduced into a vein. It shows its most marked effects about forty-eight hours after the dose. Paralyzing the motor influence of the heart through its action on pneumogastric and medulla oblongata, and that it has this action on the heart no one doubts. Dr. O'Leary described an instrument he saw used by Waltz for showing the reduction in the heart's action, and which reduced the fact to a mathematical certainty—the number of pulsations in the carotid of a dog being marked on a revolving cylinder of white paper, by means of a pencil of Indian ink. During his experiments it was especially difficult to obtain pure the digitaline, as most of what even the first chemists possessed was of inferior character, and different specimens showed opposite results.

The acid, on the other hand, in combination with the acrid oil, acts as a diuretic through the urino-genital organs; but we have before seen that digitalis, by its sedative effect on the circulation, is indirectly diuretic,

and, therefore, we have the double action—a direct one through the volatile oil, and an indirect one through the digitaline—both diuretic.

Digitaline also dilates the pupil; it does so by acting on, first, the impulsive or radiating fibres of the iris dilating it; and, secondly, on the regularizing or circular, giving it immobility; so far then for digitalis as a pure sedative and diuretic. As regards its accumulation, it has for a long time been put down as the type of an accumulative drug. That suddenly the most dangerous symptoms are produced by its continued use, by the sudden prostration in assuming the standing posture, is clear to all; but Dr. Garrod, in his recent work, appears to doubt this—I mean its pure cumulative powers—and attributes these properties to its imperceptibly weakening the heart's action, and this continuing and increasing beyond a certain point; when suddenly are felt those effects which follow its continued use. Others, as for instance, Dr. R. Macnamara, considers it is accumulated in the system, developing its poisonous qualities when that accumulation goes beyond a certain degree. The fact, at least, remains to warn the practitioner that its continued use, even in cases favourable for its administration, is attended with danger, and that his patient should be carefully watched. That indirectly it arrests plastic exudations, by lessening the force of the circulation, is evidenced in inflammation of the lungs and pleura.

We may now consider those affections in which it is more especially used in the present day. Time would not allow more than a superficial glance at all; therefore, I shall consider one or two, more particularly as those of most importance, and cursorily glance at others. In heart affections, and the two great classes of those organic in character—mitral and aortic—with or without hypertrophy, we find that hypertrophy being a natural method of cure, and seldom occurring, *per se*, but accompanied with dilatation. Here digitalis is indicated when we relieve the necessity of curing the hypertrophy, which we do when we lessen the flow of blood through a narrowed orifice, as in aortic narrowing; but in mitral insufficiency this is not our object; we want force to drive the blood to the different extremities, and then digitalis *only* can have a poisonous effect. And then arises the question how does digitalis act on the heart? All are aware of the extensive communication existing between the pneumogastric and sympathetic nerves, through the cervical ganglia of the neck and cardiac branches; the final ramification of these being entwined round and buried in the substance of the heart itself. And also of the dependence of the rhythm of the heart, and its cause being asserted to be owing to the due working of these ganglia; so many small centres, like the Fenian ones of our own day, on which depends the proper working of the *head-centre* of the heart. Now, a stimulant applied to the floor of the fourth ventricle retards the action of the heart, and effects vary with the amount of that stimulus; I mean an increase or decrease of

its action ; and looking on the rhythmical action of the heart, as dependent on the healthy state of these three parts, viz., medulla, pneumogastric, and sympathetic system ; we see how that digitalis, by its acting directly on these, will control palpitations and intermissions, functional in their character, by its influence on the ganglia in the heart ; and also that in organic disease its action on the heart, that we desire to produce, is attributable to its effect on the medulla and sympathetic nerves. Dr. Rawdon Macnamara, in his last admirable edition of Neligan, says it is contra-indicated in organic diseases of the heart, attended with hypertrophy and dilatation ; but seeing that hypertrophy and dilatation go hand in hand together, one or other in preponderance, as the hypertrophy is great or otherwise—and that if we reject its use in every case where these two are present, we shall then limit our use of it to mere functional derangements only, and diminish to a great extent the therapeutic value of digitalis. In aortic aneurisms it keeps the evil day off by its effect on the circulation. Inflammation of the lungs and pleura are benefited by its lessening the flow of blood through them and impeding exudations. In febrile conditions, and in dropsies, especially those, I think, complicated with heart affections, where a vegetable diuretic, non-stimulant in its character, is indicated. And here, I think, the infusion is the form most preferable to administer, as it contains nearly all active principles, sedative and diuretic, combining effect on heart and kidney. In pulmonary hemorrhages, too, its efficacy is undoubted. Also externally, as an application to neuralgic parts, acting, perchance, like other alkaloids, on the sentient extremities of the nerves of the part. And now we gradually merge into those nervous affections that digitalis is of use in ; but, before noticing these, I would glance, in passing, on the action of digitalis in menorrhagia, as first noticed by Drs. Robert Lee and Dickinson. They observed that it checked the bleeding, and they attributed its effect, not so much to its action on the circulation, as to a direct action on the fibres of the uterus itself. Now, we know that the involuntary fibre-cells of Kölliker compose the great bulk of the uterus, developed into non-striated involuntary, in the pregnant condition, whereas those of the heart are striated. The nerves that supply the uterus, derived from the aortic, spermatic, and hypogastric plexuses of nerves, branches from the latter, accompanying the uterine arteries, doing so into the substance of the uterus itself, ganglia being detected on these. And Dr. Tyler Smith, in speaking of the nervi-motor functions of the uterus, says :—There can be no doubt that it is supplied with incident motor nerves, and that the various reflex actions of the uterus are proofs of the communication of these nerves with the pneumogastric and other nerves. The uterus, like the heart, has been shown at times to have a peristaltic action, and like it, rhythmically, has been known to contract in a manner similar to the heart ; and Dr. Smith goes on to say, that from the connexion of the two splanchnic nerves

with various plexusses, abdominal and pelvic, it is quite possible for nerves to reach the uterus from high up in the spinal cord (and thus from the medulla itself); and he also shows how that the state of the circulation, by influencing the spinal centre, and thus the nerves issuing from it, must effect the contraction of the uterine fibres, be that an anemic or hyperemic state; in either case, increased action is the result, though in the former case the force is soon exhausted.

Now may not we attribute the action of digitalis on the uterus to a sort of double action. 1st. To the nervous influence transmitted from upper part of spinal cord and medulla, thence through splanchnic and other nerves, and through them to the sympathetic ganglia distributed on the walls of the arteries in the uterus, causing them to contract, rather than to a direct action on the muscular fibres of the uterus themselves. 2nd. By its influencing the state of the circulation in the spinal centre, and causing those fibres to contract. Dr. West says that the effect does not always take place, and that it is in cases of uncomplicated menorrhagia that it has its best effect; as also that the hemorrhage has continued unabated in some instances though the digitalis was pushed as far as prudent.

Entering into the vexed question of a classification of delirium tremens, and how digitalis can act in it, would be at present a discussion that would occupy too much time. We know, on authority we ought not to doubt, that it has been given in delirium tremens, and apparently with good effect, as by Dr. Jones, of Jersey, a greater discrepancy still existing as to what dose it should be given in, and in what particular cases it is of service. Looking at the pathological appearances after death from delirium tremens, we find that it is the result of no decided lesion of the brain. We do find congestion, solidification, serous infiltration; but these may be the result of secondary consequences of the disease itself—of cerebral or meningeal inflammation that has set in as a complication; and here, too, notice must be taken of the fact, that other diseases are likely to be present in your habitual drunkard or hardened tippler—such as Bright's—and that complicated with heart affections, gastric and hepatic, and lung derangements; and we must see that death ensues from these complications as much as from the disease itself. Looking, therefore, to the fact that digitalis, in its ordinary effects, does not tend to check the state of things existing in delirium tremens, such as the *copious urine, dilated pupil, peculiar pulse, moist skin*, but rather to increase them in that class of cases where cerebral complications are not indicated, the question naturally arises why give it? Dr. Wood says "That there is no doubt that, could we look into the interior operations of a brain of a patient in delirium tremens, we should see the springs of the organ everywhere relaxed, its machinery moving languidly and feebly. Though the apparent results at first, indeed, look like over excitement, but, on carefully considering them, we see that it is not so; the heart

may beat quickly, but it does so feebly; muscles jerk spasmodically, but it is with a universal tremor." Now, I think, with these facts before us, and the acknowledged actions of digitalis clearly laid down, it would appear that it is only in a certain class of cases that it is indicated; I mean only in those cases in which, from what I can gather, two other grand remedies are inapplicable—I mean *opium and support*. In those cases, arising in whatever manner they may, or from any cause, though there can be little doubt that external circumstances will alter both the nature and character of the delirium; let the patient even be greatly prostrated, yet, if by the scanty urine, peculiar pulse, contracted pupil, and delirium, we are afraid cerebral inflammation or excitement is running high, and that opium is contraindicated; then, I think, digitalis as a vascular sedative, perchance, or by its influence on the coats of the arteries, will have a beneficial effect; and all this time we must not forget that complications, existing in other organs, must deteriorate from its value as a remedy at one time, or augment its effects at another; as also that the dose must vary in individual cases, and that opposite results ought to be looked for from extreme opposites in the quantities administered; and finally, that the fact of alcohol causing congestion of the blood in the brain and elsewhere, a portion of it remaining unchanged in the blood, and the change in the urine in delirium tremens, are all points requiring careful consideration before venturing an opinion on its use. At least I think we may conclude that, like all asserted specifics, the danger is in too blind a reliance on its virtues rather than a careful discrimination of its bad effects.

In epilepsy it has been used successfully and otherwise; and recently Dr. Corrigan cured cases, in conjunction with Dr. Macnamara; giving it as infusion, in large doses in full, till its effects were produced on the system. In considering its action, for which I have not time now left to do so carefully, we must remember that as it has succeeded or failed so also have numbers of others supposed to be specifics in this disorder, and that various as have been the assigned causes of epilepsy so also have been remedies proposed; and whether it be that in those cases of a successful issue the result depended on some eccentric irritation, as derangement of the heart, uterus, or other organ, or that it was centric the result of over excitation of the brain and excess of supply of blood, digitalis, in the one case relieving the patient by its direct action on the deranged state of the organ, be it uterus or heart in the other, the over-stimulated brain, by its sedative action on the circulation, I think has not been up to this time exactly determined. The, as yet, complicated and feebly understood connexions of the nervous system with organic life, leaving many points, at least, guess-work, medicine not being reduced to a positive science leaves us groping as regards the administration not alone of digitalis but of other drugs. Its acknowledged

efficacy in mania, too, of late years, seems strongly to point the attention of the profession to it in many cases as a remedy worthy of trial, and statements of cases in which it has been tried would be very desirable. A medical friend related a case—one of catelepsy—that recovered, in which he gave digitalis. All these facts points strongly to digitalis as a grand remedy in that incomprehensible web of nervous disorders. I think, arriving at the last point in the consideration of this drug, we may class the discrepancies as regards its administration under three heads:—

1st. The improper preservation of, and collection of the leaves, either at a period of growth, when they are comparatively useless, and consequently rendering the preparations so, it is evident that under these circumstances half an ounce of the tincture taken might act only as so much stimulant due to the spirit used in its preparation; and the medical man, invariably continuing the use of that same tincture, would indignantly ignore the danger of its exhibition in other cases; so again various other causes may render them comparatively inert, or, on the other hand, powerful; and the practitioner who, with one infusion or tincture heroically dosed his patient, might, arguing from its efficacy, have but a sorry tale when he tried the same dose on another.

2nd. That it has not been applied strictly to those cases alone in which it is indicated, the exact conditions necessary for its administration not being properly as yet classified.

3rd. That there may be a peculiar idiosyncrasy of constitution which may render it, as in the case of other drugs, capable of being borne, on the one hand, or acting as a poison on the other.—*February 13th, 1866.*

A Remarkable Case of Pneumothorax, without Perforation, under the care of
E. R. TOWNSEND, jun., M.D., T.C.D.

Henry Welstead, aged 19, an iron-founder's apprentice, was admitted into the South Infirmary and County Cork General Hospital, February 23rd, 1865. History.—About two years ago he suffered from a pain in the left side, and spat some blood; has had a cough occasionally since, particularly at night; thinks he has lost flesh lately. About fourteen days previous to his admission a pain came on suddenly in his left side and shoulder, and continued up to the date of his admission into hospital, dyspnea and cough attending it. Symptoms on admission:—Complains of cough and great difficulty of breathing; is very feverish; pulse 110; tongue loaded; has no appetite, and sleeps badly; decubitus on the back; there is a red line round the gums, and incipient clubbing of the fingers; respiration seems to be carried on entirely by the right side, the left not expanding at all, but being bulged forwards; and the ribs, during each act of respiration, are dragged over to the right side in a peculiar manner, owing to the increased action of that side; to the eye there is a manifest

increase in size of left side, which, on measurement, proved to be larger than the right by nearly half an inch; percussion elicits a marked tympanitic resonance over the entire of the left lung anteriorly and laterally; the sound over the cardiac region is as clear as over the rest of the side; there is total absence of all vesicular breathing and vocal resonance; there is no metallic sound of any kind; heart displaced, and beating quite distinctly fully two inches to the right of the sternum; sounds perfectly natural, and cardiac dulness clearly defined on the right side, by percussion; posteriorly percussion over the scapular region of the left side gives a dull sound, with tubular breathing and obscure crepitation immediately under the angle of the scapula, and over the root of the lung—with bronchopheny; from this point downwards there is marked tympanitic resonance; there is puerile respiration over the right side, which sounds well on percussion, but is dull when compared with the left side anteriorly. From these symptoms and signs it was evident that a quantity of air occupied the cavity of the left pleura, which had compressed the lung against the spinal column into the posterior mediastinum, and had displaced the heart to the right side. Treatment:—He was ordered five-grain doses of iodide of potassium, three times in the day; to have beef tea.

February 26th.—The entire of the left side to be painted with strong iodine paint, and to take five grains of Dover's powder, twice in the day, to allay the cough and restlessness; to have two glasses of wine in the day, with beef-tea. This treatment was continued up to March 2nd.

March 4th.—Tongue beginning to clean; heart appears to be approaching the middle line of the sternum—is still very weak; the tympanitic resonance is now much less than on his admission, and his breathing easier.

March 7th.—On changing his position from the supine to the prone, the percussion sound towards the base of left lung posteriorly became somewhat clearer; while at the apex, the dulness remained the same; and anteriorly over lower lobe and precordial region, which were morbidly clear; when the patient remained supine, slight dulness became apparent, indicating the presence of fluid; in the pleura there is no amphoric sound whatever heard, and no vesicular breathing anteriorly, and no succussion sound; but the patient is too weak to try for this symptom again, at present; posteriorly there is no harsh vesicular breathing heard on a forcible inspiration.

March 9th.—Left side beginning to act; during inspiration air is beginning to pass into the left lung; anteriorly the lower ribs on left side expanding slightly during a full inspiration; the upper remaining stationary, causing a peculiarly distorted appearance, as if the sub-clavicular region was flattened; there is obscure crepitation over lower lobes anteriorly; posteriorly harsh vesicular breathing is now becoming established where tubular breathing and crepitation were previously heard.

March 13th.—The bulging of the left side has visibly subsided; the right now looks larger than the left. Measurement of left side, from centre of sternum to spine, during full inspiration, 16 inches; of right side, during inspiration, 16½ inches. He is daily improving, gaining strength, and feels his breathing less difficult every day; the feverish symptoms have disappeared; the cough and expectoration are decreased.

March 16th.—To have syrup ferri. iodidi ʒ ii. added to his iodide of potassium mixture; to omit the pills at night, and to take half an ounce of cod-liver oil twice a day; this he continued until April 11th, when, as he again complained of perspirations at night, and troublesome cough which prevented his sleeping, I ordered him morphia and gallic acid.

April 17.—The action of the left side has been gradually restored; the obscure crepitation which was first heard over the lower part of the left side anteriorly, passed gradually into well marked redux crepitus, which was again succeeded by vesicular breathing, the heart having previously returned gradually to its normal position, the sounds being quite natural; the bulging of the left side has now quite disappeared, and the morbidly clear and resonant sound has given place to a comparatively dull note, especially at the apex of the lung under the clavicle, where there is well marked tubular breathing; this was succeeded after a little time by distinct moist crepitation, heard both with inspiration and expiration; there is now very apparent flattening of left side superiorly, under the clavicle, and extending to the third rib; from this point the ribs expand well, particularly the false ribs.

April 29.—Omit wine; to have a pint of porter instead. April 30.—A blister to be applied under left clavicle. April 22.—To have an anodyne draught at night, the gallic acid and morphia pills having confined the bowels; these draughts, with cod-liver oil and iodide of potassium, and iodide of iron mixture, he continued to take up to May 20; when, feeling nearly well, and quite strong enough to go to work, he left the hospital, saying he would go to the sea-side for a month before recommencing his work. His weight on leaving was seven stone twelve pounds, having increased nearly half a stone since his admission, nearly three months before. He was not weighed until after he had been up some days, so that he must have increased more than this in weight from his first admission; but as he did not know what his weight was before his illness, we could only judge of his increase from the time he was weighed in hospital.

On hearing the symptoms and history of this case previous to his admission, namely, acute pain in the left side, great difficulty of breathing, with short cough, and considerable fever, I thought the case was probably one of pleuro-pneumonia, with effusion of fluid into the left side; and inspection seemed to confirm this opinion, as whilst the patient lay on his back there was complete immobility, with increased size of

the left side, the intercostal spaces being bulged forwards, and the heart was seen pulsating considerably to the right of the sternum; but percussion soon modified this opinion—the entire left side yielding a loud tympanitic sound, so loud and resonant that the right or sound side appeared, when compared with it, to be quite dull, although percussion over this side gave a perfectly healthy clear note when taken by itself. On applying the stethoscope to the left side of the chest, no vesicular or other breath sounds were audible anteriorly or laterally, though most carefully looked for; there was no vocal fremitus, and no amphoric or metallic sound whatever. Over the scapular region and root of the lung posteriorly there was dulness on percussion and tubular breathing, with obscure crepitation and well-marked bronchophony; these symptoms were at first very puzzling, and the conclusion I arrived at after careful consideration was that the case was at first one of pleuro-pneumonia—the pleura being more engaged than the lung itself, and having secreted air instead of fluid—which air, being generated in large quantity, had compressed the lung into the posterior mediastinum and dislocated the heart to the right side; there was no indication of fluid at all on his admission, nor did change of position alter the percussion sound as it did subsequently; this case is, in fact, almost identical with the remarkable case published by Graves in his *Clinical Lectures* (page 496, new edition), from which I take the following extract;—

“I was called by Dr. Dwyer to visit a young gentleman affected with cough and mild feverish symptoms; indubitable evidence was afforded, by the stethoscope and percussion, of a considerable portion of the lower lobe of the left lung being on the verge of hepatization, for there were dulness, bronchial respiration, and very obscure crepitus, with bronchophony, over the affected infero-posterior portion of the lungs; in no other part of the left lung whatever was there dulness; indeed the reverse was observable even in its infero-anterior portion, which gave a preternaturally clear sound, particularly in the region usually occupied by the heart; it was evident that no effusion of fluid existed in addition to the pneumonia detected at the base of the left lung; on closer examination we were, therefore, greatly surprised at finding that the heart was pushed out of its place, and pulsated quite close to the mamma on the right side. In this case the heart was dislocated, and yet not a single physical sign of the presence of fluid in the left side existed. He continues—there is no other way then of accounting for the latter phenomenon except the supposition that the heart was pushed out of its place by air effused into the left pleural sac, in consequence of a certain degree of pleurisy accompanying the pneumonia of the left lung; the physical signs such an occurrence must necessarily give rise to would perfectly agree with those observed; it is important to add, that the inflamed portion of the left lung now went through the usual process of healthy

resolution, but that the heart had regained its natural position many days before the resolution of the pneumonia was completed ; an occurrence we can readily explain on the natural supposition that the absorption of the effused air was a process more easily and readily performed by the pleura when its inflammation was cured ; there was the restoration of the lung to its original healthy structure after the pneumonia had been checked."

The progress of resolution in Welstead's case was almost exactly similar: the air was first absorbed, the heart returned to its normal position, and the lung gradually underwent resolution ; the symptoms and stethoscopical signs exactly corresponding to those exhibited by a hepatized lung undergoing resolution. Dr. Graves in his cases considered that the air in the pleural cavity was secreted by the pleural membrane itself, and was not due to perforation of the lung ; this opinion of his has given rise to much controversy. Stokes, in his work on the chest, seems to endorse Graves' opinion, and gives a case, the only one he had then met with, in which a tympanitic resonance on percussion was found to exist on the eighth or ninth day, in a case of pleuropneumonia over the antero-superior portion of the left side, where, on the previous day there had been complete dulness ; on the next day the tympanitic clearness had extended to the postero-superior portion of the chest ; the tympanitic resonance subsided in a day or two, leaving the chest again dull, with absence of vesicular murmur. He continues—the resonance in this case could not have been due to the effects of a distended stomach, as it only existed in the upper portions of the chest, and the region of the stomach was never tympanitic.

Dr. Walsh, in his work on the lungs, third edition, page 82, says, true tympanitic resonance is excessively rare over pneumonic consolidation ; and I have scarcely ever observed it at the upper part of the chest where pleural fluid had accumulated below. When such resonance occurs at the base of the chest great distention of the stomach or colon might be suggested in explanation ; but, in point of fact, the stomach and colon are rarely distended enough for the conversion of their common amphoric into tympanitic quality ; and in the only two possible instances I have observed of pneumonic tympanitic sound, the consolidation was on the right side above the liver. *Can the phenomenon depend on temporary secretion of air by the pleural sac?* An explanation of the infra-clavicular tubular note, in pleuritic effusion, originating with Dr. Hudson, has generally been accepted in this country. He looks upon it simply as the natural note of the trachea and main bronchus conducted to the surface by the pushed up and condensed lung. And I believe that the trachea does sometimes exercise this influence, just as it will where a mass of enlarged bronchial glands bridges the space between itself and the surface. But this explanation is utterly inapplicable to all cases. What

light does it throw, for instance, on the occurrence of tubular note at the right posterior or lateral base.

In the *Dublin Quarterly Journal* for 1863, Dr. Little, of Sligo, has published a very interesting and remarkable case of pneumo-thorax without perforation of the lung, rapidly consecutive on simple hyper-acute pleurisy, in which a careful *post mortem* examination showed that the lung was perfectly free from tubercle, and was quite healthy; there was no perforation whatever. He concludes his most interesting and valuable paper by remarking that he believes his case offers the first perfectly satisfactory solution of an old pathological puzzle—satisfactory in all its details, in its history, physical diagnosis, and especially in the clear and unmistakable evidence, furnished by a careful *post mortem* examination; and that, in his opinion, not a link is wanting in the chain of demonstration that in certain cases of inflammation of the pleura (which must be exceedingly rare and exceptional), that membrane is susceptible of taking on a peculiar morbid action whereby air is copiously secreted into the pleural sac, inducing a rapidly fatal form of that disease which he designates simple pleuritic pneumothorax.

In opposition to these views, I may say that Drs. Alison, Fuller, and nearly all the late writers, as well as Drs. Haughton, Williams, and many of the earlier writers on this subject, when enumerating the causes of pneumothorax, all mention the secretion of air by the pleural membrane, as one of the sources from which air may be effused into the pleural cavity—but do so solely on Lænnec's authority; and all seem to be opposed to the possibility of its occurrence, except Walsh, who admits the possibility of it in rare cases of pneumonia; this shows the extreme rarity, at all events, of such a result of pleuritis. These authors consider the cause of the disease to be perforation of the lung from some disease within the lung itself—most commonly tubercular ulceration; they are all, therefore, unanimous in looking on the occurrence of pneumothorax as an universally fatal complication, the patient usually dying within a space of a year and a-half, at the outside. Dr. Gairdner, in his *Clinical Medicine*, whilst holding the same views as to the cause of pneumothorax being almost always due to perforation of the lung, yet differs from all the others in considering that pneumothorax, especially when limited in extent, is not a necessarily fatal occurrence.

He relates a case of pneumothorax, published by Dr. Thorburn, of Manchester, in which extensive pneumothorax existed on the right side, which gave rise to very little suffering or inconvenience, and was not accompanied by any cough. The patient was a healthy young man, and never exhibited any symptoms of tubercular disease. He perfectly recovered, and his chest, when examined some months afterwards, appeared to be perfectly healthy.

Dr. Gairdner, in commenting on this case, which he had himself

examined, says, what happens in cases like this is—that the lung previously affected or not with tubercle, emphysema, or other form of lesion, weakening at points its power of resistance, gives way by a mere pin-hole perforation, through which the pleura would rapidly enough become quite distended with air were it not for the pre-existence, in some cases, and the very rapid formation in others, of pleuritic adhesions, sufficient to limit the consequences to a part only of the cavity. In most cases, too, there occurs a new pleuritic attack, the result of the pneumothorax, by which the opening, when small, is often sealed up long before the pleura has become full of air, to a degree productive of the most extreme form of suffering from dyspnea. In some instances again, and especially in those of tubercular cavities, proceeding to perforation, he has seen reason to believe that a little air may gain the cavity of the pleura by transudation without an actual perforation, the serous membrane having its vitality weakened so as to exercise little more control than a dead tissue over this mechanical transudation. He bases this inference chiefly on a single instance in which it appeared to him evident that a small quantity of air had so escaped—the case being one of tubercular disease in which pneumothorax was not suspected during life, and death seemed to have caught the disease in the very act of perforation as it were by a mere coincidence of time. In other instances he has seen two or even three successive perforations in the same lung, the last of which only has proved fatal, the others having been sealed up shortly before the death of the patient by the effusion of soft lymph. He is thus led to consider pleurisy in its relation to pneumothorax as not a fatal but a healing power, as it tends to limit the extent of the pneumothorax, and is thus an effort of nature to cure the disease by sealing up with lymph the small perforation. He refers to Graves' case also as an instance of recovery; but considers the case somewhat imperfect in its details. He continues:—"Dr. Thorburn's case is, as I have said, unique in respect of the distinctness of the clinical facts, unless it be matched with a case recorded long ago by Graves, but afterwards involved in doubt, from the objections raised by Dr. Williams to the theory of diagnosis of the Irish physician. There can hardly be a difference of opinion now-a-days as to the insufficiency of the evidence of pneumothorax in most of Dr. Graves' cases; and it is, perhaps, no less difficult to accept his theory of the simple secretion of air into the pleural sac; yet, I am much disposed to think that this one case, at least, is safe from criticism as to the diagnosis in respect of the evidence stated to exist, of displacement of the heart quite to the right nipple, and its recovery afterwards of its normal position." As there is no mention of metallic phenomena whatever, and as the case is somewhat imperfectly related, considering its vast importance, it may be, on the whole, safer to consider it as being not quite conclusive.

In my opinion the absence of all mention of metallic phenomena renders the case most important, as it tends to confirm the opinion of Graves that the case was one of secretion of air by the pleura, and not of perforation—the occurrence of the metallic tinkling being pathognomonic of that lesion; and if it had been present could scarcely have escaped the observation of Dr. Graves.

Dr. Alison, speaking of the physical signs of pneumothorax, says:—“Tympanitic percussion sound of perforation is generally accompanied with other signs declaratory of the accident; I may say that in every example other signs must have existed at one time or another, so generally is this form of percussion associated with amphoric respiration or voice, with metallic tinkle, ranging over a great area, with splashing sounds and comparative immobility, that, except in an exceptional case, which one has some little difficulty in conceiving, I should be disposed to regard tympanitic resonance, though highly suspicious, as inconclusive of the presence of perforation, without the combination of one or more of these other signs.”

The tinkle of perforation is generally present without intermission in perforation of recent occurrence; when perforation becomes chronic, and the patient lives long, which is occasionally the case, the tinkle may cease for a time, just as the amphoric blowing may do; and this I am disposed to believe, though I am by no means certain of it, depends upon the closure of the aperture, it may be temporary; either by cicatrization, or by occlusion by means of lymph or mucus; in one case both the amphoric blowing and the metallic tinkle were absent for weeks or months—they had been heard a year before death, and also immediately before it. As I have said before, neither amphoric resonance nor metallic tinkling were ever heard in Welstead's case, though most carefully looked for; I must therefore say, that after carefully considering all the arguments on both sides of this question, I have no doubt but that this case was a case of pneumothorax without perforation of the lung, although the patient was of a phthisical appearance, and had evidence of tubercle in left lung; before leaving hospital this, I believe, was deposited in the apex of the lung when condensed by pressure, and was probably induced by the pneumonia; except for the occurrence of which he might have escaped the tubercular disease.

TRANSACTIONS OF THE MEDICAL SOCIETY OF THE
COLLEGE OF PHYSICIANS.

DR. BEATTY, President.

DR. JOHN A. BYRNE read the following history of a *Case of Puerperal Fetid Pulmonary Abscess* which he had met with, and which possessed some interest on account of its rarity as a puerperal complication.

Mrs. T——, was confined on September 22nd, 1863, at 9 a.m., having been in labour about ten hours; according to the statement of the nurse-tender, the labour pains had commenced at 11 p.m. on the previous night. When I saw her at 4 o'clock a.m., the membranes were ruptured, the os uteri was fully dilated, the head was in the first position, and everything appeared to be going on in the most favourable manner; the pains still continued; and at 9 o'clock a.m. she was delivered of a very fine female child. There was some *post partum* hemorrhage, which yielded to active measures; but this did not appear to make any impression on her pulse, and she soon recovered from it, and appeared to progress very satisfactorily.

There was nothing during the whole of the second stage, which extended over about five hours, viz., from 4 to 9 a.m., that was calculated to excite any alarm or uneasiness; the pains were moderately strong. She was a primipara; and although there was not much interval between the pains, yet, the continuance of the second stage for this time did not seem to produce any symptoms of exhaustion, such as the obstetric practitioner is so often familiar with, and which cause him so often great anxiety of mind. Thus, her pulse remained quiet; her skin cool; her tongue clean; the vaginal discharge normal, there was no retention of urine; she exhibited considerable tranquillity of mind; and she was, to all appearances, a fine, healthy, well formed-woman.

I am particular in dwelling upon this circumstance as it might possibly be supposed that the train of symptoms which afterwards exhibited themselves in this case depended upon or were referable to exhaustion, produced by a too long protracted second stage, a circumstance which, as I have before stated, every accoucheur of experience must constantly meet with; but I had not the slightest uneasiness on this account, as I had not left the lady for a single moment until she was delivered, and, consequently, could form a most exact opinion as to the frequency and duration of every single uterine contraction, the progress of labour, and the effect of the labour upon the life of the child; and, I need scarcely assure my listeners that this is one of the most important duties which devolves

upon the practitioner in such cases; and that the responsibility of judging accurately, and balancing nicely the strength of his patient, and the effect of the labour upon the child's life, against time, is one of the most difficult and important functions which he has to discharge.

The lady, as I have stated, was a very fine well formed woman. Her age was about thirty; she had dark hair and dark eyes, and appeared to be of the bilious temperament. I afterwards learned that she had been under the care of an eminent medical man in this city, for an affection of the liver; and in my interview with her before her confinement, I perceived that she was very desponding and low spirited about the approaching confinement; and she told me frequently that she was certain that she *would never survive it*. I need scarcely say that I used my best endeavours to remove those impressions or fancies by laughing them off; but they continued to haunt her, and never left her for one moment after her confinement.

She appeared to be going on very well until the 24th September, the third day, when she had a slight rigor; and this was followed by pyrexial symptoms, and she complained of some pain over the left iliac and supra-pubic regions; this pain was increased by pressure; it was not, however, very intense, nor was there well-marked metritis. Fomentations and poulticing, with an opiate, having failed to remove it—and the pyrexia and uterine tenderness still continuing, she was ordered ten leeches to the uterine and iliac regions, and to be poulticed, and some grey powder with ext. opii. and James' powders, to be given every third hour.

September 25th, fourth day.—She is somewhat relieved of the pain; the pulse is 108; she did not sleep well, and there is an irritability of manner about her, and a wild appearance which made me apprehensive. I ordered her to be still poulticed, to continue the powders, and to have, at night, an anodyne, containing solut. morphisæ, half drachm; dilute hydrocyanic acid, 2 drops; camphor water, 6 drachms.

September 26th, fifth day.—She had no sleep; abdominal tenderness removed; pulse 108; pyrexia; there was some diarrhea. Ordered to stop the powders, continue poulticing, and an enema containing 30 drops of acetum opii to be given at night; chalk powder with opium to be given at night till diarrhea ceased.

September 27th, sixth day.—Had no sleep whatever; manner very wild and excited; pulse 108; talks incoherently; is very desponding; manifests no care for her infant.

On this day she complained of a stitch in the right side, at the lower part of the lung, posteriorly, and she had a slight cough: there were no physical signs of any pulmonary or pleuritic affection.

She was ordered a small blister to the side, and some extract of hyoscyamus every three hours, and a draught containing 25 drops of chlorodyne.

I proposed to her husband to have a consultation to-day, as I did not like her condition; accordingly my friend, Dr. M'Clintock, saw her with me, and we ordered her solution of morphia, 25 drops; dilute hydrocyanic acid, 2 drops; distilled water, 6 drachms—to be taken at night, if necessary.

On the 28th, seventh day.—Still no sleep. Pulse 110; there is great excitement of manner; no care for food; she talks incoherently; no abdominal pain or tenderness. I cautioned the nurse and those about her to watch her most carefully, as I suspected that something would happen; and at 8 p.m. she uttered a loud piercing scream, and became maniacal, and required the efforts of those about her to prevent her getting out of bed and doing injury to herself.

We saw her in the evening, and ordered her chlorodyne, 45 drops in draught, and to have nourishment at regular intervals during the night, in case she did not sleep.

September 29th, eighth day.—Dr. M'Clintock saw her this morning with me; she had passed the night without one moment's sleep—in screaming, scolding, swearing, and reviling all about her. She refused nourishment; in fine she was in a state of mania such as is familiar to the accoucheur, and none of the signs of it were absent. I need not say that her friends were in a state of great alarm, not believing it possible that such a train of events could take place.

She had now passed many days and nights without any sleep whatever, although anodynes had been given to her; and the necessity of producing sleep was manifest. We accordingly prescribed Battley's sedative liquor of opium, 1 drachm; antimonial wine, 1 drachm; simple syrup, half an ounce; camphor water, 6 ounces—2 tablespoonfuls to be taken every 3 hours till sleep be produced. Muriate of morphia, 1 gr.; simple syrup, half drachm; with camphor water, in a draught, to be given at midnight, if she did not sleep before that time.

Dr. M'Clintock and myself saw her at night, and up to that time she had not slept; towards midnight, however, she became more tranquil; but the nurse to whom we had given express directions not to give the draught if she were better, did give it to her, and on our arriving the following morning we found her completely narcotized; the nurse informed us that she had been in this state for some hours; and we then became aware of the fact of the draught having been given, contrary to our express directions; she lay breathing heavily, her pulse was slow, her pupils contracted, her face congested; she was fully under the influence of opium; and it required long-continued efforts on the part of Dr. M'Clintock and myself to rouse her from this condition, by mustard, emetics, slapping of the hands, soles of the feet, and the administration of brandy, &c., &c.

After some time we were successful, and she became restored; and from

that moment she lost all her *maniacal symptoms*, and she slept. She, however, continued depressed, and she complained of pain in the right side, where the blister had been applied; this pain had been masked by the occurrence of the maniacal symptoms, and as soon as they subsided it again developed itself.

She had a troublesome cough. On examining the chest we could discover nothing wrong, except some slight dulness over the inferior part of the lung, and some mucons râles.

She was ordered another small blister and poulticing to the side, with anodyne expectorants, and claret, and beef-tea were freely administered.

The cough continued; from this period it was of a short hacking character; her breathing, occasionally hurried, at no time presented dyspneal characters; nor was there any physical sign either of pneumonia or pleural effusion. She slept at night, except when disturbed by cough; there was slight expectoration occasionally, and she usually lay on her right side; her pulse, however, retained its character; she began to suffer from night perspiration, and her face had a hectic flush; and she complained of nausea, which was produced, as she said herself, by a horrid smell coming into her throat. This was not perceptible to the attendants for some days, but gave herself considerable annoyance, and created a repugnance to food.

This state continued for several days; when on October 17, at midnight, she was attacked by cough and great dyspnea; and these symptoms were followed by the discharge of a most fetid expectoration. This continued during the greater part of the early morning, and on my first visit the nurse showed me a large vessel nearly filled with it. On entering the room the fetor was so abominable that I was nearly sickened by it.

Character.—The matter expectorated was of a greenish colour, of the consistence of thick cream, and resembled pus discharged from an abscess. There was no appearance of blood either then or subsequently in it, but some dark-coloured streaks were apparent, and the fetor possessed an indescribable character. It had a most penetrating odour; to my sense it appeared more like the smell of putrid macerating flesh than anything else; but indeed I am not aware of any smell to which I could compare it. The nurse stated, that after the cough and dyspnea had continued some time she threw up this matter, and it appeared to give her much relief.

The nature of the case was now, indeed, manifest, and I had the advantage of a consultation with Dr. O'Reilly, who agreed with me in the view which I took of it.

The treatment it is scarcely necessary to detail at full length; it consisted of wine, ample nourishment, poultices to the side, and antiseptics.

October 28.—During the last twelve days there has been very little change. Occasionally she had a remission of the symptoms; then she took considerable nourishment. She slept well on some nights, and for some days

she would remain almost free from cough, dyspnea, and expectoration ; but her face began to assume a hectic appearance ; her pulse remained always above 100, and the intolerable smell always permeated the atmosphere of the room, and the fetor was always most offensive to herself. She would seem to all appearances to be getting better, when she would suddenly be attacked by cough, vomiting, great difficulty of breathing, and she would then expectorate a large quantity of this horribly-smelling matter. This would be followed by great exhaustion, which would subside, and for some days she would remain cheerful, free from cough, and only expectorating small quantities of matter ; and this went on.

About this time a small subcutaneous abscess showed itself on the inner side of the knee, which, when opened, discharged a healthy-looking pus. She was removed to the country.

October 29.—On this day Dr. Stokes saw her in consultation with me. Her condition was as follows :—

She had been in the country for a few days, and appeared to have derived some benefit from the change. However, on the evening of the 28th the dyspnea and cough returned, and the paroxysm was so violent that the attendant thought she would expire, when she ejected the full of a large tea-bowl of matter. This was partly by vomiting and partly by coughing. It was retained, and shown to Dr. Stokes, who was at once struck by its appearance, and the peculiar fetor.

She then obtained relief, and at our visit she appeared progressing.

Dr. Stokes examined her chest most carefully, and the physical symptoms did not reveal much. There was only dulness over the inferior part of the right lung, and some mucous râles ; there were no signs of enlargement of that side, nor any of compression of the lung, nor of consolidation, nor of large cavity. Respiration was heard over the greater part of the lung, and râles over part of the left lung. Her pulse was 120 ; her respiration tranquil ; her skin covered by greasy perspiration. She sat up in the bed and conversed cheerfully. Her appetite is good ; she takes wine, porter, bark, &c., &c., and as far as appearances would indicate, she does not suffer from such a terrible disorder.

In examining her chest a small abscess was seen over the lower part of the spine.

It is not necessary to peruse the history of this case further. It is sufficient to say that she gradually sank, and died on 25th December, just three months from the date of her confinement.

Her death was the result of exhaustion, produced by such a long continued drain upon her system, and she expired after one of the attacks of dyspnea, and expectoration of fetid matter.

There was no autopsy permitted, so that I had no opportunity to detail the condition of the lung, &c., &c.

Remarks.—The case which I have related to the society, in my opinion,

possesses many points of interest—and with its permission I will direct attention to them as briefly as possible:—

1. The mania being symptomatic of blood poisoning.
2. The sudden cessation of it.
3. The occurrence of the phlebitic abscess in the pulmonary textures.
4. The termination in gangrene of the lung.

Although real mania did not develop itself until the seventh day, yet one of the most characteristic symptoms was present almost from the commencement, viz., sleeplessness, and required the exhibition of opiates. There can be no doubt, when we take into account the rigor, the pyrexia, the pain over the lower part of the uterus and iliac regions, of the existence of inflammation in the appendages of the uterus; this yielded very quickly to treatment, and mania showed itself whilst mischief was being brewed in another quarter.

This mischief was evidenced by the cough, pain in the side, &c., and by subsequent events.

This symptomatic mania, although not of very frequent occurrence as a puerperal complication, yet does sometimes happen. Drs. Hardy and M'Clintock mention some cases, viz., Case XXII., page 62, in which fatal erysipelas, after labour, was ushered in by mania.

Case XXVI., page 64, in which mania ushered in an attack of metritis.

They also mention another case in which mania completely masked the existence of fatal puerperal peritonitis.—Page 71:—

“There is a fact of much practical interest connected with our present subject, which we may here allude to, namely: that abdominal inflammation sometimes alternates with the alienation of reason, and this at a period after delivery when a patient is usually considered free from the liability to such attacks. A remarkable instance of this was related to us by Dr. Johnson. A patient some days after delivery became maniacal, and in this state continued for a week, when she regained the possession of her intellect, but to this immediately succeeded violent peritonitis, which carried her off. A nearly similar occurrence took place in another instance, whose history we have given at length under the head of Convulsions, Case No. 8. In Case XXVI. of the present chapter it may be observed that, after the patient was attacked with mania, the symptoms of uterine inflammation almost entirely disappeared, but upon the return to consciousness they again became apparent. We lately saw a fatal case of puerperal mania, of the low, melancholic form, with rapid pulse. On examining this woman after death, there was found most extensive peritonitis, with copious lymphic exudation, although during life she had not exhibited a single symptom to lead to a suspicion of the existence of abdominal inflammation. This remarkable case was also seen by Dr. Johnson and Dr. Denham.”

Drs. Sinclair and Johnston mention a fatal case in which it preceded, by some hours, fatal coma, where a cerebral abscess was found on autopsy. —Page 63. Also a case in which it ushered in fatal double pneumonia.

Dr. Tyler Smith also speaks of it as occasionally masking uterine or peritoneal inflammation.

Burns, in his work on midwifery, alludes to this in speaking of the origin of mania. He says, in some instances the delirium is connected with the state of the uterus, particularly of its veins, which are inflamed. There is fever, accompanied by delirium, which is the prominent symptom, so that the primary cause is overlooked till the belly becomes tympanitic; and presently, after this, the patient falls into stupor. At the first we may ascertain that there is more or less tenderness on pressing on the hypogastrium.

So also Montgomery mentions a case in which metritis coexisted with this form of mental derangement. And in the works of authors allusion is made to some instances; but in most of them the uterus or its appendages were in a state of inflammation; however, in this case, at the time that violent mania exhibited itself, the uterine symptoms had disappeared, and there was no hardness, nor enlargement, nor pain; nor was there subsequently any secondary deposit in or near the uterine region; in fact the mania showed itself first at the time that the phlebitis was beginning—if we may look upon phlebitis as the cause—to produce the changes in the pulmonary structures which afterwards manifested themselves in so peculiar a manner. If then it be presumed that phlebitis was the cause, as soon as the pulmonary tissue was selected for the further development of the puerperal poison, we may suppose that either from the effect of the opiates exhibited, or from causes into which we could not see, and over which we had no control, the maniacal symptoms disappeared, as from this moment she was perfectly free from any.

In the majority of cases of puerperal mania there is generally no physical cause that we can discover, but the disease appears to be connected with, or to depend upon, the irritation produced either by exhaustive labour, the attempt at lactation (hence it has been often called “mania lactea,”), of which I saw a very remarkable example when assistant in the hospital, which I well remember; in this case it followed secondary hemorrhage, of a most aggravated character, after labour; but the woman recovered from it, and she, indeed, has since borne children and never had any return of it. Or it may depend upon some peculiar idiosyncrasy or organization, and in some instances upon hereditary tendency or taint. Thus, Gooch says, “of the cases which I have seen a large proportion have occurred in patients in whose families disordered mind had already appeared”; and this remark of Gooch’s is confirmed by the experience of many other observers.

Now although this is a mere surmise or presumption as to the manner in which, in this particular case, this phenomenon was produced, yet there are many circumstances to warrant us in, at least, not discarding it as unworthy of credence; it is no more wonderful than the effects produced upon the blood in the first instance, and then upon the nervous structures by other poisons.

If this view be correct then we are furnished with another explanation as to the cause of this curious complication or sequence of parturition in some cases, and it may be of use in forming an opinion as to the probable termination of it. We know that in the majority of cases in which puerperal mania occurs the patients remain for a considerable time before the integrity of the mental faculties is restored, whereas when there is merely a temporary delusion, and where it depends upon some dyscrasia, it may pass away soon, although the cause producing it may be of sufficient severity to destroy life, either rapidly, as in the worst forms of puerperal fever, or slowly, by the formation of abscesses in the several cavities or articulations.

The next point in this case to which I would take the liberty of directing the attention of the Society is the occurrence of a collection of matter in the pulmonary textures, and the gangrene, which must necessarily have taken place in the structure of the lung in order to impart the odour so characteristic of this disease; those two lesions of the lung are in themselves of very rare occurrence, as an effect of ordinary inflammation of the lung, but I am not aware of a single instance on record in this country of a case of even ordinary purulent deposit in the lung dependent upon phlebitis occurring after labour, much less one in which this was accompanied by sphacelus of the lung itself, although I need scarcely say that the history of puerperal diseases is most extensive, and that almost every phase of the affections comprised under the head of "puerperal" has been most minutely described.

In this case the pulmonary symptoms commenced with stitch or pain in the side, short dry cough, and it more resembled a case of pleuritis than one of pneumonia, but at no period was there evidence of pleural effusion, although there scarcely can be a doubt that the pleura was very much engaged; nor was there evidence of pneumonia in its first or second stages, either by physical or rational signs. It is possible that the matter expectorated in such quantities may have been secreted in an interlobar cyst, but it is perfectly certain that there was no evidence, at any time, of effusion between the costal and pulmonary pleura, or of communication between the pleura and bronchiæ, giving rise to the phenomena of pneumothorax.

My friend, Dr. W. Moore, has lately contributed a most valuable and instructive paper to the association, in which he has detailed the history of a very remarkable case of pleural fetid effusion occurring in a

constitution broken down; and in this case the symptoms of effusion were very distinct, there being lateral enlargement, extensive dulness, absence of vocal fremitus, and respiration, and cardiac displacement, and the expectoration having a most fetid character, and in which those signs gradually disappeared on the ejection of the fluid secreted by the pleura. In this case there was no evidence of a communication between the pleura and the lung, and Dr. Moore accounts for the removal of the fluid by the explanation of transudation.

Any gentleman who has not had the advantage of hearing Dr. Moore's paper, I would refer to its published account in the 78th No., May, 1865, of the *Dublin Quarterly Journal*. Dr. Moore has given a very clear and able *résumé* of the subject, which has added to the diagnosis of pulmonary and pleural affections of a gangrenous nature.

In this case which I have read, there was, as I have said, no evidence of pleural effusion of either a diffused or circumscribed character; and I state this not having relied upon my own unaided opinion, but it being also the opinion of Dr. Stokes, who saw her on two occasions along with me, and most minutely explored every region of the chest upon both occasions of his visits, and coincided with the view which I entertained as to the nature of the affections, viz., that it was a collection of fetid or gangrenous matter occurring in some part of the lung itself. I need scarcely say to the members of this association that any opinion emanating from such a distinguished authority is entitled to the gravest consideration.

In his work upon Diseases of the Chest Dr. Stokes has detailed four cases of this affection succeeding pneumonia, and in all of them there were signs of a cavity, and the diagnosis was verified by autopsies. Dr. Stokes says:—"I have not found any peculiar physical signs in the gangrenous abscess of the lung. I believe that the only pathognomonic symptom is the extraordinary and disgusting odour of the breath and expectoration, making the patient loathsome to himself and all around him; the stench, however, is not constant, for during the progress of a case it may disappear more than once; in some cases the expectoration is fetid, while the breath is free from odour, and it will be often necessary that the patient be made to cough in order to produce the stench."

That phlegmonous pulmonary abscess is very rare may be believed when Lænnec stated that he had only met with six cases, and the same may be said of the experience of other physicians. Thus, Aitkin, Watson (except in cases where it occurs in connexion with phlebitis) and Walsh, all unite in expressing the opinion that pulmonary abscess is extremely rare, at least in these countries (although Rokitansky states that it is by no means unfrequent in Vienna); but the coexistence of a purulent collection, with gangrenous walls surrounding it, as it may fairly be presumed was the condition in the case which I have related, is an event still more rare.

Of the phlebitic origin of the case I think we can scarcely have a doubt,

and that the phlebitis selected the lung in preference to other organs is not to be wondered at; however, I have never seen this before to such an extent. All accoucheurs are familiar with cases (in severe puerperal epidemics) where symptoms of pleuritis and bronchitis manifest themselves at a very early period of the affection; and I could relate many instances which I have witnessed in hospital of persons who were carried off rapidly by pulmonary congestion and bronchial effusion; at least such were the signs during life, and the appearances after death justified this opinion.

In some of these cases metritis, or metro-peritonitis, preceded or followed by rigors, ushered in this attack, and in others this thoracic complication appeared to set in very suddenly, without much premonitory symptoms, and very quickly carried off the patient; it is not at all impossible that in those cases phlebitis may have been the cause of the rapid termination. It much more frequently happens, however, that the phlebitis manifests itself by deposits in the different articulations, and the elbow, the knee, or ankle joints are attacked by arthritis, which generally terminates by the formation of purulent deposits. Nay, even it attacks the structures of the eye; and I remember very well seeing a case in which a purulent deposit existed in the anterior chamber of the left eye, in connexion with puerperal arthritis of the left knee. This is a very rare place for the disease to exhibit itself, and it occurred in the case of a patient whom I was summoned to see, in consultation, a few years since, and who afterwards fell a victim to this puerperal arthritis. I have never seen a similar case in hospital practice, although the other form of arthritis was present in some of the cases which I have witnessed.—*May 30, 1866.*

Notes of a Case of Recovery from Traumatic (?) Tetanus. By S. M. M'SWINEY, M.D., Physician to Jervis-street Hospital.

On the 5th of February, 1866, a lady called at my house to consult me about her son, E. G., aged ten years. She did not see me, however, as I was professionally occupied out of town during that day, and did not return until a late hour—too late to comply with the request made by the lady, in a message left for me, that I would visit her that evening. I received an account that the boy was seriously and strangely ill, and I made it a point to visit him at an early hour next morning.

His appearance, as I entered the room in which he was placed was very striking and remarkable, and served to reveal but too plainly the nature of his illness. His back was turned towards me, and he was endeavouring to stand, supported by the nurse, against whom he was leaning with both his forearms. His whole frame was fixed rigidly, as if he were petrified; his legs were wide apart; his knees bent; his back greatly curved forward; his head very much bent backwards, at the same time that it was drawn nearly down upon the

left shoulder. When I changed my position and stood in front of him, the appearance of his face was extremely characteristic. It was so much altered from its normal state that, although knowing him very well, I could scarcely recognize any resemblance to his former self. His face was very pallid and damp with perspiration; his forehead was corrugated into many vertical and transverse wrinkles; his eyebrows were elevated, and at the same time drawn in towards the nose, which organ had a *pinched* appearance, whilst its alæ were dilating and contracting in unison with the respiratory movements, which were hurried and catching. His mouth was contracted and *pursed* in, its angles drawn in also; a quantity of white, frothy saliva was adhering to the lips and trickling down the chin. But I think the attention of an observer would be chiefly arrested by the appearance of his eyes—mine certainly was—which had seemingly become smaller, were fixed upon one in a furtive, rat-like manner, and afforded a typical example of what the late Mr. Colles, in his description of the “tetanic face,” was in the habit, I believe, of calling “the peering eyes of tetanus.” I felt the muscles of the legs, thighs, abdomen, and neck; they were, most of them, firmly contracted and almost as hard as stone,

The *sterno-cleido-mastoids* were very tense; the *right* one, in particular, was stiffened and bellied out to a most remarkable degree. The muscles about the cheeks and mouth, too, were rigid, and—whilst there was not absolute *trismus*—these muscles, by their contraction, prevented the teeth from being separated to a greater extent than from *one quarter* to *half* an inch. Through the slightly separated teeth the tongue could be seen much swelled and thickly coated. Two or three ulcerated spots could be seen on it, where it had been wounded by the teeth. The boy swallowed with great difficulty, and very unwillingly, each attempt at deglutition generally causing a violent paroxysm of convulsions. And this brings me to speak of the exacerbations of the disease. They came on at somewhat irregular intervals, often from every *ten* to every *twenty* minutes; sometimes the intervening period would be much longer than this, and occasionally even shorter.

Whether awake or asleep the advent of a paroxysm was announced by a fretful, complaining cry, and an urgently-expressed desire to be held up. The back was greatly curved; there was, in fact, complete *opisthotonos*.

He seemed to suffer severe pain during the time the spasm lasted, a period usually of 12 or 15 seconds, after which he returned to his previous state of abated or mitigated suffering. He had a troublesome, short, hissing cough, caused, apparently, by the rapid accumulation of a viscid mucus about the pharynx, which he wished to get rid of, and which slavered his teeth and lips. His voice was reduced to almost a whisper, and even an attempt to *speak* seemed to cause him considerable distress. His pulse was very fast, usually 120.

The *history* I received about this boy's illness was as follows:—He had been noticed to be not quite well *six* days before the time of my visit. He was pale, slow, and, as it was thought, stupid, in his manner. His tutor complained that he would not learn his lessons; he was even chided for playing antics and making grimaces; he walked in an odd and somewhat constrained manner, as if he were stiff from some cause or other. His appetite was failing, and at night he was restless and feverish. But no particular attention was paid to him until about *three* days before I saw him, when his mother thought he had *rheumatism*, and gave him some mild medicine. From that time his symptoms increased in severity until they arrived at the height already described as their state when I first visited him. There was no recent cut or abrasion on any part of his body, nor was there any history pointing to such. Two occurrences, however, either, or both of which, had, I think, exercised an important influence in the production of this attack, had taken place about a fortnight before he was seized with his illness; *one* was that he had slipped when going down the steps leading from the hall door to the street, had fallen back heavily, and was pained and stunned for a short time. The *other* occurrence took place about the same time as the fall, and was this:—In the course of some school game, or play, he had carried a boy, older and much weightier than himself, on his back for some time. He was obliged to desist, however, from sheer fatigue and inability to continue, and he was noticed to be pale and exhausted after this exertion, during which, I have no doubt, his spinal cord received some hurt. I had no difficulty, as may be easily supposed, in diagnosing the nature of this boy's illness, and I concluded that it was of the *traumatic* variety. There was, to be sure, no wound with a solution of continuity, but such is not requisite to constitute *tetanus* "traumatic." A blow, bruise, or violent concussion may, it is known, produce this form of *tetanus* without causing a division of any of the tissues, and the great severity of the symptoms and the rarity of the idiopathic form in these countries, formed considerable elements in addition to my estimate of the influence exercised by the above-named occurrences in bringing me to this conclusion. I had no reason to suspect the existence of intestinal worms—by far the most frequent cause of the *idiopathic* variety—nor could I surmise any other cause of reflex irritation. Still, as a preliminary proceeding which might help to determine this question more certainly, I at once threw up a strong turpentine and oil enema, and gave a smart calomel and rhubarb purge. The bowels were shortly moved, and acted well some *four* or *five* times, but no worms were expelled. I explained the gravity of the case to the parents, ascribing the attack to injury of the spinal cord, inflicted upon either of the before-mentioned occurrences, and I advised a consultation to be held. Dr. Banon was associated with me, and had no hesitation in confirming my diagnosis. We held out no—or at least but very faint—

hopes of recovery, and we settled the plan of treatment to be adopted. I will not weary members by describing minutely the progress of this case day by day. It will be enough to mention that for the first *three* days he appeared frequently in danger of immediate death. On the *third* day he vomited during an attack of unusual severity, and was thought to be about to die. The *masseters* and *buccinators* were rigid on this day, and actual closure of the teeth seemed imminent. He rallied, however, and on the *sixth* day the severity of the symptoms was moderated. On the *eighth* day it was noticed that he was comparatively free from *severe* spasms during the day, but still suffered very much during the night. On the *twelfth* day my note is :—"Much improved to-day ; was not so bad during the night, and slept tolerably well. Muscles less hard ; is able to open his mouth wider and swallows better ; his countenance is more natural ; his recovery may now be reasonably expected."

He was so well on the *fifteenth* day I did not consider it necessary to visit him on the next day, and on the *nineteenth* day, although still weak and tottering, I was able to pronounce him to be, in all important particulars, well.

With the reading of my notes of the history, symptoms, and termination of this case of acute tetanus, I have well nigh exhausted what I had to say in the communication which I have the honour to make to the Association. But to render the description of the case more complete, some mention of the nature of the *treatment* had recourse to must now be made. Dr. Banon and I agreed, in consultation, not to adopt any so-called "heroic" method of treatment, but rather in our management of the case to be guided by the principles of common sense, experience, and the known action of remedies on the living, human body.

Starting with the proposition that the disease was one of a morbidly excitable condition of the spinal cord, arising from some abnormal irritation, and producing convulsive spasms, which tended to destroy life, either by *apnea* (which might occur at any moment) or by, from their violence, wearing out speedily the strength of the patient, we decided upon a treatment which should have a two-fold object, viz., *first*, to allay or mitigate the violence of the convulsions by the application of counter-stimulation along the course of the spinal column, and the administration of some of those substances known in medicine as "anti-spasmodics ;" and, *secondly*, to sustain the energy of the muscular system by the frequent and persevering exhibition of invigorating and nutritious substances. We had scarcely the faintest expectation of being successful, but we considered that a treatment deduced from these views afforded the only possible chance of a cure. Strong animal broths were directed to be given, as well as he could be persuaded to swallow them, at frequent and regular intervals. Beaten-up eggs were prescribed for him. He was ordered to have from *six* to *twelve* ounces of wine in the *twenty-four* hours, in half-

ounce doses. And as his power of deglutition was seriously interfered with, we directed that *four* ounces of the strongest beef-tea, with from *two* to *five* grains of sulphate of quinia, should be regularly thrown up the rectum every *six* hours, and every effort made to retain it there. In addition, *milk*, in any quantity he could be got to take it, was allowed.

The drugs employed were—*chloroform*, *tinctures of belladonna*, *Indian hemp*, *opium*, *hyosciamus*, and the preparation of ether, known as "*Hoffman's Anodyne Liquor*."

A draught, consisting of a proportion of one or more of these preparations (just as, at the time, appeared judicious) was given, at intervals of about *six* hours night and day. The effect was anxiously watched and studied; and the dose was fixed, as nearly as could be, at the point where seemingly it could exercise its sedative or controlling effect on the convulsions. And as observation would appear to warrant, sometimes one, sometimes another, of these preparations would be omitted or substituted, as the case might be.

Vesicating collodion was applied along the spine, from the neck to the sacrum. I think a point of considerable importance in this treatment was, that the nourishment and remedies were directed to be given, in suitable quantities, at comparatively short and regular intervals, night and day. I strongly impressed the value of this course upon those in attendance upon the boy; and I must say that I ascribe the successful result, in a great measure, to the fact that the treatment was scrupulously and perseveringly carried out to the letter. The boy's mother, and an excellent nurse, never left him night or day, and gave draughts, nourishment, and enemata with the utmost regularity and exactness. I may, perhaps, be allowed to mention, that I myself often stayed a long time with him, introducing slowly and with great difficulty, through the almost locked teeth, a quantity of one or another of the nutritious or medicinal substances named above; and, taking a deep interest in the case, I made a point of giving him, twice every day, for some time, an enema of strong beef-tea, with four or five grains of *quinia* in it. Success ultimately waited upon this combined plan of treatment; his strength was well sustained by *the food*, whilst the dreadful spasms were somewhat held in check by the *narcotics*, until, finally, their energy was altogether worn out and overcome. I do not propose to enter into any speculation as to the essential nature of tetanus, an obscurity—which remains yet to be dispelled—admittedly surrounds its pathology. I content myself, on this occasion, with presenting a succinct report of the facts, as far as I observed them, of the case I have just detailed.

On Insanity and Criminal Responsibility; a Commentary on the Autobiography of a Criminal Lunatic. By THOMAS MORE MADDEN, M.D.; M.R.I.A.; Licentiate of the King and Queen's College of Physicians in Ireland; Member of the Royal College of Surgeons of England; Licentiate of the Faculty of Physicians and Surgeons of Glasgow; Demonstrator of Anatomy, Carmichael School of Medicine.

A REMARKABLE case of insanity, leading to a double attempt at murder and suicide, which came under my notice in May, 1865, led me to devote consideration to the medico-legal relations of insanity in reference to criminal responsibility. The result of this inquiry has been to show me in how unsatisfactory a state our criminal jurisprudence is on these points; and I now call attention to this case for the purpose of pointing out the defects of the present system, and of offering some suggestions for its improvement.

On the 22nd of May, 1865, J. E., aged eighteen, an inferior employé in a public office, who had only, within a period of six or eight weeks, recovered from fever, was observed by a medical gentleman in the office to present a wild and restless appearance, and was recommended to go home and remain there for some days; accordingly, he returned to his mother's house, at Drumcondra, where nothing peculiar in the lad's conduct was observed by his family. At the expiration of some days he returned to his employment, stating he was quite well. The gentleman above referred to again observed the same expression he had before noticed, and called the attention of the senior clerk of the office to it desiring that the boy should be allowed to leave the office earlier than usual, or for some days more, if he wished, to remain at home. He left that day at the usual hour; conducted himself quietly during the evening; but at night, when all the members of the family were asleep, he arose and made his way to his mother's room, on entering which, he flew at her—then in bed—and attempted to kill her with a hatchet, which it was afterwards ascertained had been concealed by him several days previously; but her cries, fortunately, brought timely assistance, and she escaped with some slight injuries. Before the lad was seized, however, he inflicted a slight wound upon himself, in the right breast, with a clasp knife; threw himself on the ground, roaring vehemently, and endeavoured to dash his head against the wall. Having been secured, he was carried to his bed, when, after some hours, however, feigning sleep, he took advantage of the inattention of those who watched him, and, early next morning, made his escape from the house and flung himself into the River Tolka, near his place of abode, in Clontarf; he was pulled out, with some difficulty, by a policeman.

Next day he was brought before a police magistrate, charged with the

attempt at murder and suicide, and was committed to prison. By the exertions of Dr. R. R. Madden, however, he was, ultimately, sent to the Richmond Lunatic Asylum, placed under the able care of Dr. Lalor; he was discharged, as cured, in November last, and is now earning his bread comfortably and creditably in another public office in this city.

This young man was a pale, cachectic lad, of emaciated habit and leucophlegmatic temperament, badly brought up, ill-educated; his only reading being the cheap *sensational* tales and novels published in the London penny serials, to the perusal of which, notwithstanding various remonstrances and admonitions of his superiors, he devoted every spare moment.

After this outbreak of insanity, which I have described, a quantity of papers and fragments of papers, in his hand-writing, were found in his desk, written in the sensational-novel style he delighted in. Amongst these manuscripts was one document, entitled, "*Autobiography of J. E.*," written immediately previous to his attempted matricide; and I am now induced to publish a few extracts from this paper as a curiosity of psychological literature. These fragments furnish the best comment I have ever seen on the effects to be apprehended from the prevalent sensational cheap literature of the present day.

"—*Autobiography of Joseph E.*—Joseph E. was born on the 20th of March, 1846. He displayed in his infancy a quick, volatile, proud disposition, and, although this was seen by his mother, she never took any pains to eradicate it, and let him go on without doing anything towards removing this grievous evil; but she was a poor struggling woman, and had her husband lying ill for months and months on his back, and she had no means of support only merely what she would earn by her own industry. There was never in this world a more industrious woman in the world, though she never took any pains to inculcate this virtue into Joe, for she thought as she acquired this industry naturally, as she thought herself, she imagined Joe could not be otherwise; so, thus, the best boy in the world was ruined by being let go and take his own head and habits in all things, for his mother thought all was over when she sent him to school.

"You never, I suppose, thought it necessary to know whether he minded his prayers or not, or religious duties or not; oh, 'twas terrible! oh, horrible! horrible! Oh, how could God stand it and let the child live and go with such wings to the devil? Oh! never, never, will she forgive herself for this. But to return, as well as my poor head can let me, unfortunately as it proved he was sent to a small National School when six years of age, and his disposition may be proved by an instance which I may lay before the reader, if ever this finds one. . . . Oh, reader! never take pleasure in reading those things; never soil your hands with them, but of dire necessity as a warning, and take care you take it in that light. Oh, my God! my God! never,

In the same desk as the foregoing insane autobiography there were also found several other documents which were equally characteristic of the writer's insanity. Amongst these were the following fragments of unconnected observations not of the same date, probably, as the preceding rhapsodies :—

Q 2

enthusiastic child; of a strong and healthy frame, although his father was a poor, sickly, weak man.

“Oh! with what a quick hurried bound he leaped into school; no hesitation, no drawing back then; no, no, all straight; head up, and ready to look the world in the face. Oh! with a little care what fine stuff was there to be moulded to a good shape, but it was let run to rust, and now it is gone completely to ruin. Oh, poor unfortunate child! oh, infernal mother! oh, mother! who being blessed with a son never once in his infancy held up his childish hands in prayer to the great God of all glory. Oh, villain! worse than the most *diabolical*.”

The handwriting of the preceding rhapsodies, though very legible, is peculiar. The words run closely into each other. There are no stops, no space left unwritten, no margin left unscribbled on the sides of the paper. The absence of margins and stops has been noticed in other cases of documents written by the insane, and has been commented upon; among others by Olivier d'Ange, and Bayard, in the *Annales d'Hygiène*, Vol. XIX., p. 490.

In this case we find a boy originally of a low order of intellect, of limited capacity, vain, frivolous, and of weak resolution, whose education and moral discipline evidently appear to have been neglected, and whose only reading was the lowest class of *sensational* literature, full of thrilling tales of crime and mystery. This kind of reading engaged his entire leisure, and evidently filled his imagination; so that like Don Quixote he surrounded himself with a world of fiction, which shaped his subsequent mania. Thus the autobiography, the attempted murder, and abortive suicide, all bear the impress of this mental dram-drinking furnished to the poor in the London penny sensational periodicals, and to the rich in more pretentious sensation novels and magazines.

The ill effects of the long continued and exclusive perusal of such literature, especially on young minds, are similar to those described by Fabius:—

“*Educatio altera natura; alterat animos et voluntatem; atque utinam (inquit) liberorum nostrorum non ipsi perderemus, quum infantiam statim deliciis salvimus; mollior ista educatio, quam indulgentiam vocamus, nervos omnes, et mentis et corporis, frangit: fit ex his consuetudo, inde natura.*” —Lib. I., cap. 3.

I have heard it said that the influence on the particular forms of crime which have been ascribed to novels is as imaginary as are the scenes depicted in those works themselves. But that such is not the case we have abundant proof. Madame de Staël assures us that the *Sorrows of Werter* occasioned a vast increase in the number of suicides in Germany at the time of its publication. Anyone who has had occasion to turn over the police reports of the London newspapers of some forty years ago, will find this influence of literature shown in the number of foolish youths

charged each morning with emulating the exploits of the heroes of Mr. Pierce Egan's then celebrated *Life in London*. Some years later Mr. Ainsworth's *Jack Sheppard* seems to have excited a highwayman epidemic among the lads in many a country town. And we have reason to suppose that a similar influence is still exercised by the penny serials, such as *The Boy Pirate*, *The Young Highwayman*, and the cheap and coarse stimulating literature of the penny weekly journals, to which the individual on whose case I am commenting, abandoned his feeble mind.

This case is a good example of the *manie raisonnée* of Pinel supervening on fever. There seems to have been no hallucination, but, simply, a perverted affection which led the boy to suppose that, as his mother had, by neglect of her duties, religious and maternal, imperilled her salvation, it was his duty to save her from eternal retribution by depriving her of time and opportunity for incurring further guilt by killing her, and then committing suicide with the view of joining her in the next world. This *manie raisonnée* has its analogies with Bardolph's reasoning in his lamentation for his dead master:—"Would I were with him wheresome'er he is—either in heaven or in hell!"

Had this boy succeeded in his attempt on his mother's life he would have been tried, as a matter of course, for murder; and if the plea of insanity were set up we should, in all probability, have had abundant sarcasms about "the pseudo-sentimentality of the mad doctors," and what one journal, commenting on a trial, not long since, elegantly designated "the moral insanity dodge for cheating the gallows." We would have found medical men, who had seen the patient, testifying their belief in the perfect sanity and consequent criminal responsibility of the writer of the preceding diary. Very possibly, too, he might have been tried by a judge who disbelieves in partial, or so-called "moral," insanity, and a jury who believe, of course, whatever they are told by the judge; and the wholesome and salutary spectacle might have been exhibited of a human being publicly put to death because suffering from the most dire disease that can afflict human nature.

The term "moral insanity," which many might apply to the foregoing case, is often and, I think, justly objected to; but the idea intended to be conveyed by it, of insanity marked by hallucination of the moral faculties or affections, without any *obvious* derangement of the intellectual powers, unquestionably applies to many cases. Every one conversant with the forms of insanity must have met cases in which a sudden perversion of feeling sets in and hastily changes the natural disposition and character of individuals whose minds still preserve the semblance of reason, while their moral conduct or natural affections become so unaccountably changed. The phrase itself, "moral insanity," seems to me a most unhappy one, however. For, although cases do sometimes occur, as within the last few weeks we have had a notable instance, in a recent

trial in Dublin, in which disorder of the moral faculties alone is apparent, it does not follow that disease of other mental faculties does not co-exist with this moral disorder, though not so evident. Moreover, the very serious objection has been urged that the idea of "moral insanity," if widely extended, would lead to the fearful doctrine, that crime is generally the result of an involuntary and irresistible mental malady by which all freedom of volition and of action are destroyed, and, that crime and madness being synonymous, men are, consequently, not responsible for their evil actions.

Instead of the numerous terms used to describe the various types and forms of insanity—and which seems to me, although, doubtless, of value to the psychological physician, calculated rather to embarrass and perplex than to aid the medical witness in courts of law—I would venture to suggest that, for medico-legal purposes, unsoundness of mind, not including mental deficiency or idiocy, should be divided into the two classes only, of general and partial insanity; the latter being the only one in which medical evidence is needed in cases of crime ascribed to insanity.

A madman is, it may be presumed, one in whom the faculties, or any one of them, which should regulate and point out his relations and behaviour towards God, his neighbour, or himself, are either lost or impaired by disease.

Obviously, such a person cannot be considered as either morally or legally responsible for his actions. For to be responsible for an act it is essential that the person committing it should possess liberty of will as well as of action, which a lunatic does not enjoy, or he would be none.

But besides the state of mind in which a man is responsible for his acts, or sanity, and that condition in which he is not accountable for them, or insanity—there is a third condition of mind in no way provided for by our law, and which seems not sufficiently recognized even by the medical profession. I allude to what Baron Von Feuchtersleben terms, "a state of half freedom."^a That is a state of transition between the healthy and unsound mind, either preceding or following insanity. In this state the patient is only partially able to exercise self-control, and therefore is but partially responsible for his actions.

^a This peculiar condition of mind should be recognized by law in this country, as it is in France, where on a jury bringing in a verdict of "plus innocent que coupable," the *Advocat-Général* may order an investigation into the state of mind of the prisoner, and award a punishment in proportion to the real guilt of the accused.

(To be continued.)

Wat: Dun President

My dear friend, I am very sorry to hear
of the unfortunate accident to your horse.

Memoir of Sir Patrick Dun (Knt.), M.D., sometime President of the King and Queen's College of Physicians in Ireland; including his Will, his Deed for Constituting a Professor of Physic; and Several Important Records concerning the Profession of Physic in Ireland, never before published. By T. W. BELCHER, M.D., Dublin; B.M. and M.A. Oxon. and Dublin; Fellow and Censor of the King and Queen's College of Physicians, &c.

INTRODUCTION.

DURING the last year I published a memoir of Dr. Stearne, Founder and First President of the Irish College of Physicians; this was so favourably received by the learned body to which I have the honour to belong, that, at their request, it was re-printed, in a separate form, from the *Dublin Quarterly Journal*;^a and was considered by some friends, in whose judgment I place confidence, to give a fair idea of the state of the profession of physic in Ireland during the latter half of the seventeenth century, as well as a true account of the ancient relation of the College of Physicians to its Alma Mater, Trinity College, Dublin.

The present memoir will, in effect, continue the history of the College of Physicians from the death of Dr. Stearne, in 1669, to our own day; and thus, beside placing on record all that I have been able to collect about a remarkable man whose name is familiar to us in connexion with his medical bequests, it will, I trust, in some degree serve the important interests of medical education in Ireland. Beyond a short sketch of Dun, which appeared in the *Dublin Quarterly Journal*, in 1846, from the pen, I believe, of Mr. (now Sir William) Wilde, and a prior notice of him which the late Professor Osborne published in his *Annals of Sir Patrick Dun's Hospital*, in 1854, nothing has been hitherto done, so far as I have ascertained, to obtain and publish full and accurate information about a man whose memory well deserves to be cherished with respect by the citizens of Dublin, and by the medical profession in Ireland.

In the following memoir I have inserted all the material proper to such a work, which I could obtain after diligent and long-continued search. In it will be found several original and important public documents not hitherto printed, as well as private letters of Sir Patrick and Lady Dun, and other curious matters.

AUTHORITIES.

I may briefly note the chief authorities used or quoted in this memoir:—
Mr. Wilde's *Memoir*, already quoted; Professor Osborne's *Annals*, &c.; Dr. A. Smith's *Early History of the College of Physicians in Ireland* (*Dub. Jour. Med. Sci.*, vol. xix.); Mr. Wilde's *Contributions to the History*

^a For May, 1865.

of Medicine in Ireland, Dublin, 1846; *Various Statutes of Parliament*; *Journals of the Irish Houses of Parliament*; Dr. W. D. Moore's *History of Pharmacy in Ireland* (*Dub. Quart. Jour.*, Aug., 1848); Journals, and other books and papers of the K. and Q. Coll. of Physicians, and pamphlets in the Library there; MS. petitions and minutes of committees among the Parliamentary papers in the Record Office, Dublin Castle; Barrett's MS., described in my memoir of Stearne, and kindly lent to me for this purpose by the Rev. Dr. Todd, S.F.T.C.D. Beside all these, I have particularly to acknowledge the kindness of Sir Bernard Burke, Ulster King of Arms, who not only allowed me to read the records of Civil Affairs in the minutes of the Irish Privy Council, and other documents kept in the Record Office; but who, himself, in various ways facilitated my work, and gave me much original information, which is duly acknowledged in the following pages. I have further to thank Dr. Lyons, one of the present Censors of the College of Physicians, who kindly placed at my disposal a collection of original MS. letters addressed to Archbishop King, which had come into his possession; some of these, written by Sir Patrick and Lady Dun, appear in this memoir. Further, I must thank Dr. Dyce, of Aberdeen, for the loan of a privately-printed memoir hereafter quoted, and for several letters and papers of value, which appear or are referred to by me; some of these he obtained from gentlemen to whom I also give my best thanks, viz.:—The Rev. William Barrack, Rector of the High School, Aberdeen; the Rev. Archdeacon Bisset, of Lissendrum, a collateral descendant of Dun; and John Stuart, Esq., Keeper of the Records in the General Register House, Edinburgh.

THE FAMILY NAME. •

With reference to the spelling of the names I may observe that every possible variety seems to have prevailed. In the following remarks it will appear spelled as Dune, Dwne, Dun, Dunn; in fact there seemed to be no authorized spelling of the family-name; and it must be remembered that at this time (1642–1713) men altered all these things at pleasure, and with a frequency which we know nothing about now. In the *Dublin University Calendar*, for the present year, the editor kindly added a note of mine, under the head of "School of Physic," to show that Sir Patrick never spelled his name in any other way than as "Dun." He signed himself at various times as "Dun," "P. Dun," "Pat Dun," and "Pa. Dun;" but in no instance did he sign his surname as "Dunn."

DUN'S BIRTH AND FAMILY HISTORY.

The subject of the present memoir was born in Aberdeen, in January, 1642. His father, Charles Dun, litser (or dyer), was a burghess of Aberdeen, and Patrick was his second son by his second wife, Katherine

Burnet. This branch of the family of Dun had been then long settled in Aberdeen, and belonged to the ancient stock of Dun of Dun, or of that Ilk, near Montrose.* Charles Dun was one of the nephews of Dr. Patrick Dun, Principal of Marischal College, and the munificent endower of Aberdeen Grammar School, where his name is to this day held in great respect; and where his portrait (by Jamieson), presented by his representative, Archdeacon Bisset, may be seen.^b

In a letter from the Rev. William Barrack, Rector of the school in question, to Dr. Dyce, of Aberdeen, dated 3rd February, 1866, the following passage occurs:—

“There were certain Registers kept in the Town House which might have thrown some light on the subject, but these have all been removed to the Register House, Edinburgh; but I have no doubt Sir Patrick’s name will be found in the Register, and the exact date of his birth will thus be ascertained. Dr. Patrick Dun, who bequeathed the lands of Ferryhill to the Grammar School, must have been his grand-uncle, for in the bequest he says, ‘After my decease I nominate, constitute, appoynte, and ordayne the Provest, Baillies, and Councell of the said burgh of Aberdeen, for the time, with Maister Robert, Charles, and Patrick Dunnes, my brother sonnes, and their aires male of perfyte aige, they being known to be honest and conscientious, and not given to railing, to be Patrons and Conservators, *in perpetuum*, of this my mortification.’ The will is dated 1631, August 3, so that Sir Patrick, born in 1642, must be a son of one of these three, the nephews mentioned above, and, therefore, grand-nephew of Dr. Patrick Dune, as he spells his name in the will. There are no records in the Grammar School, and none in Marischal College; but there is no doubt Sir Patrick first attended the one and then finished his studies in the other; after which he probably proceeded, like other learned Aberdonians of that day, to some continental university, to finish his medical education. Dr. Liddel studied at Heidelberg and Rostock, and was Professor at the Julian University, Helmstadt, where Dr. Patrick Dun studied.”

Mr. Barrack then proceeds to say that the Duns of Tarty, the family locality of Dr. Patrick Dun, the grand-uncle of the subject of this memoir, have ceased to exist.

The records to which Mr. Barrack refers were examined, as appears by the following letter from Mr. Stuart, Keeper of the Records, General Register House, Edinburgh, to Dr. Dyce:—

* “Of which family was the famous Duns Scotus.”—Nisbet’s Heraldry, 1722.

^b Charles Dun (Sir Patrick’s father), died in 1667.—Osborne’s Annals of Sir P. Dun’s Hospital. Dublin, 1844.

“GENERAL REGISTER HOUSE,

“EDINBURGH, 8th Feb., 1866.

“DEAR SIR,—The Registers to which you refer are not in my charge, but my friend Mr. Patton, in whose department they are placed, has examined them for sixteen years, and the result is the enclosed extracts.

“I trust that they may be useful. I have looked into our Spalding Club books, but see no notices of Dun. In the Poll Book of 1696, in the parish of Logie, Buchan, I see the family of Tarty are enumerated, but I do not know their relationship to your man.

“I am, very truly yours,

“JOHN STUART.

“You will see that *Dr. Patrick Dun* was one of Patrick Dun's godfathers.

“J. S.”

The following is a copy of the extracts enclosed by Mr. Stuart; they are interesting as showing the way in which such matters were recorded in Scotland at that time, particularly as regards the spelling. It will also be noted that the Dun family belonged to the then Established Church of Scotland—the Episcopal.

“Anno 1638 yeares. Charles Dūne & Cathren Burnet, mariet the penult day of December.”

“21 October, 1639. Charles Dwne and Cathreine Burnett, ane sone, his name Andrew. Bap. be Doctor James Sibbald, Doctor Williame Johnestoune, Mr. Robert Farq^r., bailzie Alexander Jaffray, Andro Burnett, and Alexander Ramsay, Godfatheris.”

“31 October, 1640. Charles Dune and Kathreine Burnett, ane dochter namitt Kathrein. Bap. be Doctor Wm. Guild. Mr. Alexander Jaffray late Prowest, Doctor Robert Dwne, Daniel Aidie, Thomas Burnett, William Innes, and Alex^r. Burnett, Godfatheris.”

“13 Januarie, 1642. Charles Dwne and Kathreine Burnett, ane sone namitt Patrick. Bap. be the said Mr. Androw; Doctor Patrick Dwne, Mr. Johne Osūall, Mr. Patrick Chalmer, John Jaffray, Wm. Cutbert, and Patrick Fork, Godfatheris.”

“9 November, 1645. The said day Charles Dunne & Kathreine Burnett ane sonne named Hew. Baptised be Wm. Robertsons. Rober Taylour, &c., Godfatheris.”

“13 December, 1649. Charles Dunne and Catherin Burnett, ane daughter Jeane. Mr. Alex. Irving, Minister, &c., Godfatheris.”

(From Registers of City of Aberdeen, searched till 1654.)

Several interesting particulars respecting the family of Dun and their descendants, are recorded in a valuable privately printed book, of which

Dr. Dyce, of Aberdeen, kindly gave me the perusal.^a In note G., page xii., will be found notices of the family of Anderson, of Bourtie, and of the family of Ross, of Arnage; and in note U., page xlii, some curious information is given respecting Principal Dun, and the family of Johnston—all closely connected with Sir Patrick.

In this book, among other curious matters, it is stated that Christian Mitchell, widow of Charles Dun, litser in Aberdeen (who had died in April, 1581), was burned in that city on the 9th March, 1597, “on various charges brought against her of sorcery and witchcraft—the most of which were, that by administering drinks Christian Mitchell had caused the death of her own son, ‘Andro Dwn,’ and her ‘awin dochter in law—vmquhill Maray Johnstoun.’” It is not the least singular circumstance connected with that affair, that the unhappy woman confessed the truth of the accusation brought against her before the magistrates of Aberdeen, “as ane commoun witche and sorcerar, be oppin voce and commoun fame, sua reput and hauldin thir xxviii yeris bygane, be vsing of witchecraft and sorcerie, be the inspiratioun of the Devill.”—“In signe quhairof, the Devill gaf the a nip on the bak of thy richt hand, for a mark that thow was ane of his numer.” The writer then proceeds to state, that about the close of the sixteenth century the citizens of Aberdeen were possessed with a mania on the subject of witchcraft, and he refers to Vol. I. of the *Miscellany* of the Spalding Club (1841) for information respecting that epidemic; at the same time remarking that at pages 164, 165 of the volume in question will be found a copy of “the particular dittay and accusation, confessit be Christen Michell, relict of vmquhill Charlis Dwn, litser, burges of Abirdene, quhairvpon the said Christen is accusit as a notorious witche and sorcerar, in using the craft and tred thereof, be the inspiration of Sathan, thir many yeris by past.” This unfortunate Christian Mitchell, or Dun, whose only crime, in all probability, was that she was old and ugly, was paternal grandmother to Principal Dun, and, therefore, a relation of Sir Patrick in the direct line.

HIS PROFESSIONAL CAREER, AND CONNEXION WITH THE COLLEGE OF PHYSICIANS.

I have searched in vain for any particulars of Sir Patrick's early career; and the first notice of him, so far as I know, occurs in a letter written from Dublin Castle by Sir John Hill, to Lord Forbes, then of Culloden, near Inverness, father of Duncan Forbes, who, by his wife Mary Innes, daughter of the Laird of Innes, in Morayshire, was, in his

^a A short Memoir of James Young, Merchant Burgess of Aberdeen, and Rachel Cruikshank, his Spouse, and of their Descendants. With an Appendix, containing Notices as to the Connections, by Marriage and otherwise, of many of that Family. Compiled from Authentic Sources, and now printed for the information of the said Descendants, in regard to whom the Details have been brought down to the Year 1860.

turn, father of the celebrated Duncan Forbes, Lord President of the Court of Session. Hill writes^a (as quoted in the *Culloden Papers*, published at London in 1865, page 10), under date 14th February, 1676 :—
 “Here is one Dr. Dun, an Aberdeensman, who is Physitian to the State, and to my Lord Lieut., desires to have his service remembered to your son, Duncan, with whom he had acquaintance in Paris.”

From this letter it is plain that Dun was at one time abroad ; and it is also highly probable that, as suggested by Mr. Barrack, he studied medicine there. At any rate he made a good position for himself in a short time, for at the date of Sir John Hill's letter he was only thirty-four years of age, and then he had, in all probability, made his fortune. As a practitioner in Dublin he appears to have been highly esteemed ; and his personal and social qualities must have been thought much of, for the next notice which I can find of him refers to his election to the Fellowship of the College of Physicians, the number of the Fellows being then limited.^b

He was probably chosen a Fellow in 1676–7, that is, between January 1st and March 25th, 1677—new style.

In D'Olin's book, referred to in my Memoir of Dr. Stearne (p. 16, note), among the entries of Dr. Willoughby,^c who was Treasurer from 24th October, 1677, to 18th June, 1680, is this statement :—

“De Doctore Dun pro admissione sua, £10. 00s. 0d.”

This is without date,^d but it precedes an entry which is dated Feast of St. Michael, 1678.

On the 24th of June, 1681, he was for the first time chosen President of the College of Physicians, probably on the death of his predecessor, Dr. Robert Waller ; and he seems to have been annually elected to this office up to St. Luke's Day, 1687, when Dr. John Crosby was elected President. I shall shortly refer to the circumstances connected with Dr. Crosby's election—but, meanwhile, I may add that Dun appears to have again filled the highest office in his College in 1690, 1691, and 1692. On the 15th of December in this last year he was nominated the first President under the new College Charter of William and Mary ; and he

^a Memoir of James Young, &c., *supra cit.* Note G., p. xii.

^b See Charter of Charles II., in my Memoir of Dr. Stearne.

^c Charles Willoughby, or “C. Willughby,” as he signed himself in some letters of his which I have seen, was one of the Fellows of the College of Physicians, nominated in the Charter of 1667, and also in the charter of 1692. He was a physician of note in Dublin, and died in 1694. He left his library to T.C.D.

^d The date of payment of fees is no fair index to the date of admission to Fellowship at this time, as in some cases the fees were paid when the person was made a candidate, and in others not until long after admission to Fellowship. The amount was often paid in two instalments, and sometimes a bond was accepted for the debt.

also was chosen President in the years 1693, 1696, 1698, and 1706.^a So far as I have been able to ascertain, Sir Patrick Dun's contributions to medical literature were very few—the title of one, only, has come down to us, viz., a paper “On the Analysis of Mineral Waters,” read before the Dublin Philosophical Society, of which he was one of the founders in 1683.^b He was, however considerably in advance of his age in regard to practical anatomy; for it is stated that “the first record of a public dissection in this city, is that made by Mr. Patterson in 1684, of the body of a malefactor procured for that purpose by Sir Patrick Dun.”^c ●

Of the original members of the Dublin Philosophical Society, Mr. (now Sir Wm.) Wilde, wrote:—“These men formed the *stellæ majores* of Irish literature and science at this period, and nearly every one of those of whom we have any subsequent account attained to considerable eminence either here or in England.”

The very fact of Dun having been associated with Archbishop Narcissus Marsh, Provost Huntingdon, Sir Richard Bulkeley, Archbishop King, Bishop Ashe, Judge Loftus, Archbishop Palliser, Bishop Smith, and Sir Thomas Molyneaux, says much for his ability and energy of character, and gives, in a great measure, the key to his early and long-continued success in life. As noted with reference to another eminent physician,^d it was not then the custom for medical men to write much on their own profession. They read much and wrote little, and what they did write was mostly on general literature, or, as was frequently the case with physicians at that period, on theology or ethics. If we consider the widely-spread prejudice which existed with regard to the practical study of anatomy in Dun's time, we shall be able, to some extent, to appreciate his public advocacy of anatomical science, and his known practice of it himself. His views on this subject will more largely appear on perusing his intentions as to the ultimate disposal of his property; but, meanwhile, it may well to say that, in the College of Physicians of Dun's earlier years, Trinity Hall,^e anatomy, though pursued under difficulties which would damp the ardour of many a student in our day, was studied, and paid for, too. In fact, the practical anatomy of Trinity Hall seems to

^a Register of the King and Queen's College of Physicians in Ireland, &c., 1866. Pp. 101, 102. In this book I have given all the authorities for the above statements, so that they need not be here repeated. See Preface to Appendix, p. 99.

^b For a full account of this notable Society, the first of the kind in Ireland, see Mr. (now Sir William) Wilde's Contributions to the History of Medicine in Ireland. Dub., 1846 (reprinted from Dub. Quart. Journ. of Medical Science, Feb., 1846). In this Society Dun appears to have made powerful friends.

^c Memoir of Sir Patrick Dun. Dublin Quarterly Journal of Medical Science, Vol. ii., 1846, p. 290.

^d See my Memoir of Dr. Stearne, p. 38.

^e For the history of Trinity Hall see my Memoir of Dr. Stearne.

have, to some extent, served the purposes of our modern Dublin Pathological Society.

Among the archives of the College of Physicians is a small book, bound in vellum, marked on the cover, "No. 7. Some old accounts," and inside, "An Account Booke of Receipts and Disbursements for the Colledge of Physitians, beginning Jan. 21, 1672." This record is evidently in the handwriting of Dr. Crosby, whose name has been already mentioned, and among the entries are the following:—

"It. to ye Joyner for ye dissecting table, the 15th of March, 1676,	6s.	0d.
"It. to ye Cutler for cleaning ye instrumts. belonging to ye College,	5s.	5d.
"It. to ye Carpenter for altering ye window shutts,	2s.	0d.
"It. to ye Sexton of St. Andrewes ^a for keeping the dogg, Sixpence,		6d.
"It. for a Warrant for ye body it was dissected,	1s.	3d.
"It. to ye Souldiers who kept ye body,	4s.	6d.
"It. for ye Coffin for ye sd. body,	4s.	6d.
"It. to ye Souldiers who watched,	9s.	0d.
"For the said Souldiers in drinke,	3s.	10d.

"The whole sum spent on ye same bod[y] being £2 4s. 10d., I delivered upon ye President's note unto his man."

This record shows one of the uses made of the College of Physicians when Dun was in a good position in Dublin. I assume that previous to his election to a Fellowship, he was a Candidate, *i.e.*, one chosen to fill a vacancy in the Society when such should occur; but he must have been not only a Fellow but a leading man in the College on the 4th of April, 1677, for under that date the Treasurer entered in the book from which I have just quoted:—

"It.: Payd. Mr. Boldey, of ye Castletauern, for a treatate giuen Dr. Dunn vpon his address to ye College of Physitians agt. Lewis the Summ of 10s. 6d."

There is no record of Dun having taken any degree in the University of Dublin. I have searched the Rev. Dr. Todd's MS., "Graduati Dublinienses," from 1660 to 1712, but Dun's name is not to be found in the list. This, however, does not prove that he did not graduate there, as Dr. Todd's MS. list does not profess to be strictly correct, so far as including in it every actual graduate.

^a Trinity Hall was close to St. Andrew's Church.

As I have elsewhere remarked, it was customary at this time to go abroad for medical education and graduation, which was then a much easier process than such is now; because, at that time, Latin was the common language of learning of all kinds; and lectures could be heard and books read by an Englishman just as well at Utrecht as at Oxford. From Munk's Roll of the London College of Physicians, it appears that it was the almost invariable rule for those admitted to the Fellowship in that body, about this period, to have received their chief medical education abroad; and, on their return home, to have been incorporated, or admitted *ad eundem*, into one of our universities. As the Dublin College was founded on the London model, it is fair to suppose that the London example in this respect was followed here also; and, indeed, from Dr. Todd's list of graduates, it appears that this was actually the case in several instances, so that it may be presumed that Dun was, in like fashion, admitted *ad eundem*, or incorporated, at Dublin; and this supposition derives additional probability from the fact, that in his portrait, by Sir Godfrey Kneller, which is now in the college here, and to which further reference shall be made, he is depicted in the robes of a Doctor in Physic of the Dublin University; and from another picture of an M.D. of the Caroline period, to be seen in the Provost's house, it is plain that our medical custom and that in Dun's^a time are identical.

From the earlier minutes of the College of Physicians it is apparent that Dun was a leading man in its concerns from the outset of his connexion with it. On all occasions of importance he was one of the few selected to represent the college before other public bodies, or in communication with the Government. Among other offices which he filled may be noted that of Censor, to which he was elected on five separate occasions subsequent to the issue of the present Charter of 1692: in 1694, 1695, 1697, 1699, and 1707.

Mention has been already made of Dun's friendship with certain remarkable personages in Church and State. Among these Archbishop King,^b as he afterwards became, seems to have been his chief favourite. Dun was King's medical adviser; and, as will afterwards appear, the Archbishop was one of those to whom Dun entrusted the guardianship of his affairs by will. From the following letter may be seen Dun's plan of giving advice and gossip at the same time:—

“Reverend Sir,—I received yours of the 19 and 26, I am of

^a See Register of the College of Physicians for 1866, p. 26.

^b William King—born at Antrim, 1650; Chancellor of St. Patrick's, Dublin, 1679; Dean, 1688; D.D., 1689; Bishop of Derry, 1690; Archbishop of Dublin, 1702; ob. at his palace, St. Sepulchre's (now a police barrack), 8th May, 1729; buried at north side of “churchyard of Donnibrook, near Dublin.” See his life in “Harris's Ware (Bishops).” He founded Archbishop King's Divinity Lecture in the University.

Sr. Thomas Millington's^a opinion that it is proper for you to drinke the waters, but cannot conceive why you may not drinke the bath also you have both time and season for both, when you see Sr. Thomas againe present my service to him, present my humble service to Sr. John and his Lady, she is certainly very good and mercifull if she can enduere to see any of you for your leaving her in Chester exposed to all the misfortuns that maye befall a traveller, when you see Sr. Robert Hamiltone tell him I receaved his letter, And that the committe have reported a Physitian necessary and that my Ld. Deputie told me that his Father might nominate whom he pleased, I praye you Sir faile not of your promise to faile often [*sic*] Mrs. Crofts left Dublin the 22d being teusdaye, the fridday after she was landed att Beumorrice theye were gone from Dublin before I received your first Mrs. Kerney is with her Mr. Kerney was maide King att arms Sunday last and his Sone herald the purchased it from Mr. St. George.

"I ame Sir

"Your faithfull DUN.

"June 2nd 1683.

"To the reverend

"Mr. William King,

"next door to the Cock in the Hay Market

"London."

The correspondence with King was of a very friendly kind, as may be seen from the tenor of the preceding letter. In the following year (1684) part of Dublin Castle was burnt. Dun was in court secrets at this time, having been Physician to the Lord Lieutenant, at any rate, since 1676, as appears by Sir John Hill's letter; and it is plain that he was looked on and acted as Physician to the State in 1676, although the office of State Physician did not regularly exist until 1725.^b At any rate Dun was duly informed of everything that occurred about the Irish Court; and, as King was also of a gossiping turn, his medical friend kept him up in the latest news of things going on at home.

The following letter is curious in a historical as well as in an antiquarian point of view:—

^a Sir Thomas Millington, M.D.; educated at Westminster School; Fellow of All Souls, Oxon., 1659; Sedleian Professor of Natural Philosophy, 1675; Fellow Royal Coll. Phys., Lond., 1672; Knighted, 1679; President R. Coll. Phys., 1696 to his death. His praises were sung by Garth in his "Dispensatory;" and Sydenham spoke of him in terms of the highest respect. He died 5th January, 1704, ætat 75. He was one of the Original Members of the Royal Society; and was Physician to William and Mary, and Anne.—See Munk's Roll, R. Coll. Phys., Lond.

^b Dr. Osborne in his "Annals" *supr. cit.*, says that Dun was State Physician; but Dr. A. Smith in his letter, now in the Archives of the College of Physicians, states that that office did not exist until 1725.

"Reverend Sir,—I have heard my selfe some false circumstances about the burning of the Castle in Dublin, therefore I give you the following account thereof: the fire began in my Lord Deputy's dressing room, in the new buildings built by the E. of Essex, the Crakling noise and smoake of it weakened my Lord Deputy^a out of his sleep, he gott up and was goeing out his usuall waye out of his bed chamber by his dressing room, this dressing room is in the east end of those buildings a back room betwixt the bed chamber and the castle wall, upon opning the dore of that dressing room, so much smoake and heate struk my Lord in the face that it almost took his breath from him, whereupon my Lord retreated in his shirt toward the other door out of his bedchamber and called out fire fire, sent to save his daughter and sent for powder to blow up his closet and long gallerie, to hinder the fire from goeing toward the northeast tower which looketh into Cork house Exchange and is full of powder, before this was done all those new buildings were burnt and my Ladys drawing room in which she receaved the Ladys, was in fire, to stop the progress of the fire towards the dining room, my Lord Deputy ordered the presente to be blown up, Mr. Robinson was not in town, my Lord and Mr. Cuf were the ingeniers, which was done and stopt the fire from fireing the dining room there was burnt and blown up, the new buildings built by the Earle of Essex, my Lords closet and the long gallerie and all betwixt the new buildings and the tower on which the Clock stood, the fire began on Sunday betwixt on and 2 in the morning its progress was stopt about 4, but my Lord Deputy was not drest till about six, all his cloaths were burnt, and furniture of those new buildings, Mr. Kingdon and the Earle of Longford furnished my Lord with linning and a suite of Clothes the City of Dublin had a greate deliverance, by the powders being saved for which we are all bound to bless God I present my service to Sir John Parker and his lady Mr. Kerney King at Arms was Knighted Sunday last.

"I ame Sir

"Your humble servant DUN.

"April 8 1684.

"To the reverend William King

"at Sir John Parkers house

"in Formoyle

"to be sent by the post to

"Lainsburrow."

Drinking the waters at the wells near Wexford was a fashionable medical pastime at this period, and, of course, Dun had to send his patients there, and give them advice too. The following letter has not

^a Richard Butler, Earl of Arran, second son of James Duke of Ormond.

any address on it; but it was probably intended for King. The next, dated 3 Aug., 1686, very fairly illustrates the medical practice of Dun; and also shows how he got into and retained the good graces of influential friends. The "Mr. King" mentioned in this, and in the third letter, dated 28 April, 1688, was the future Archbishop's brother, Mr. Robert King, of Skinner-row.

"Reverend Sir,—This is only to introduce the bearer hereof Mr. Sample to your acquaintance and because he is a stranger, to beg of you the favor; to be assisting to him in providing a lodging for a gentlewoman whom I have advised to drinke the waters, Captaine Morrice told me this morning that he had secured a lodging for his Lady I suppose it may be in the same house where you are if ther can be no other lodging got, att lest endeavor that this gentlewoman maye have Madam Morrice lodging till she come, by his discourse with me, I suppose she will try the new found well att Chapel Izod. I promised to give her a visite to morrow I suppose then she will fixe her resolution, this daye my man receaved the inclosed from the post office, I left my patient very well yesterdaye, she had only the measls, present my most humble service to my Lady Cole,^a I have not forgot my promise either next week or the week following I intend to see her Ladyshipe att the wells,^b I wish you all good success.

• "I ame Sir

"Your most humble servant

"DUN.

"Dublin, June 6, 1684."

"Sir,—I am very glade that you gott so well to Wexford I hope that your cough you had after your first and second dayes drinking will be much abaited before this come to your hands, I praye you not to be discouraged with the first rencounter, for many have not only found themselves disordered at first but all the time theye drank ye waters and yet have found greate benifite afterward I desired you to take halfe ane ounce or ane ounce of the syrup of buckthorn in the first glass of your waters if they did not pass, according as you found there was more or less necessity of purging and to drink the waters after as if you had taken no such thing I praye you lett me heare oftner from you, and if

^a Lady Cole—probably Elizabeth, *d.* of Jno. Chichester, Esq., of Dungannon, and wife of Sir John Cole, Bart., of Newland, Co. Dublin, and M.P. for Co. Fermanagh; or her *d.*, Elizabeth Cole, who *m.*, in 1671, Sir Michael Cole, Knt., son of Sir John's elder brother, Sir Michael Cole, M.P. for Enniskillen, and ancestor of Wm. Willoughby Cole, the present Earl of Enniskillen.

^b Wexford water, a chalybeate. Fashionable invalids resorted to the wells in Dun's time, and for many years after. See Ratty on Mineral Waters, p. 143. Dublin, 1757.

your stinging scant waters cannot inspire you in a morning, I hope after dinner when you have drunk a glass or two, of good Claret (if the town can afford it) you may find some matter to write, particularly about our Bishop of Raphoe^a whom I have recommended to your directions, as my Deputie, as also our Lady Mayoress,^b Mrs. Saffield my patient, Mrs. Dean, to whom with my most humble service I wish much joy of her new Sister, to the widow and her Daughter, I charge you upon your &c. not to say a short grace for her, but bring her to Dublin safe and single as you found her at Wexford so help &c. yesterday Mr. Johnston was with me, we discoursed about the house, after he had heard all I had to say, that we had nothing to say to the house on the terms sent to us from the Naas &c. and that we had given leave to Mr. King to take it in his own Name that he had sent a lease to Mr. Leigh with a letter of recommendation from Mr. Whitshed &c. he told me that what he had signed in our favors he would stand to it if we required it but prayed that we would not oblige him to extend it to any other, and in short gave me to understand that he had a mind for the house himself I waved giving him any answer, Mr. King is acquainted by this post, I present My Most humble duty to My Lord Bishop of Raphoe his Lady our Lady Mayoress, Mrs. Saffield, Mrs. Dean and the Widow, make ready your treat for Dr. Willoughby and myself when we come I wish you all good success.

"I am Sir

"Your most humble and faithful Servant,
"DUN.

"Dublin 3 August 1686.

"To the reverend

"Mr. William King,

"At the wells near

"Wexford."

"Reverend Sir,—I hope this may find you in good health which is my particular prayer, I am now at Moyra with Sr. Arthur Randon I came to this place yesterday being Friday, in the afternoon, this day being very cold and wet my Lady would not let me go, I told her if she kept me this day I would not go on Sunday, on Monday God willing I leave this and know nothing to hinder me, but a passing visit to Dr. Warren 4 Miles from this and another at Drogheda to my Lord Forbess I hope at furthest I may dine with you on Wednesday, I visited

^a William Smith, D.D.; born at Lisnagarvy, Co. Antrim; educated at T.C.D.; Vicar of Ardnogher, Dio. Meath; Dean of Dromore, 1673; Bishop of Killala, 1681; translated to Raphoe, 1681; and thence to Kilmore, 1693. Ob., 24th Feb., 1698, and buried in St. Peter's Church, Dublin.—See Harris's Ware.

^b Wife of Sir John Castleton, Lord Mayor of Dublin in 1686.

in my ramble Dr. Warren, the Bishop of Drummore^a who asked kindly for you and drank your health, Sr. John Mc Gill, Sr. Wm. Franklin James Hamiltone of Newcastle, who now liveth att Bangor he often remembred you, James Hamiltone of Kilmore, Sr. Arthur told me he had a letter from Mr. King since his returne from circuite present my service to him and his wife and Daughter to Dean Leslie, my Lord Bishop of Kilmoore,^b Mr. Bonell.

“I am Sir

“Your most humble and

“Faithful servant

“Dun.

“Saturday 28 of April 1688.

“To the reverend

“Mr. William King

“To be left att Mr. Robert Kin’g house

“in Skinner Row in

“Dublin.”

At the time of the Revolution of 1688, Dun was a partisan of the winning side. He got appointed “Physician to the Army in Ireland,” and, in that capacity, he accompanied King William’s army, and was present at some scenes of historic interest. His friend King was now a Doctor in Divinity, and Dun appears to have written to him an account of everything that occurred, judging from the following letters:—

“Reverend Sir,—I gott to the Camp on Munday night late and could not find Sr. Robert Suthell,^c on teusday morning early we were commanded away with the army designed for Waterford, it was surrendred yesterday. the King went thither, but the Irish were not marched out, the King did not goe into the toun, he rid about part of the walls and marched away to Major Gen. Kirk’s tent dined there, returned to Carik whether, I could not gett before this night, just as I came Sr. Robert received ane express from England I could not gett access to speake with him, I delivred your letter to Mr. Waller who promised to deliver it the first opportunity, the news by the express is that the french are on their

^a Capell Wiseman, D.D., son of a Baronet in Essex, Fellow of All Souls Coll. Oxon, and Chaplain to Arthur Lord Capell, Earl of Essex and Lord Lieut. of Ireland, who was his godfather. Promoted to the Deanery of Raphoe, and thence, in 1683, to the See of Dromore. Obiit. 1695.—See Harris’s Ware.

^b Wm. Sheridan, D.D., born at Togher, Co. Cavan; educated at T.C.D.; Dean of Down; Bishop of Killaloe, 1669; translated to Kilmore, 1681. His elder brother, Patrick, was Bishop of Cloyne. Wm. Sheridan was deprived of his See as a non-juror, and lived in London many years afterwards. Obiit. 1716.—See Harris’s Ware.

^c Principal Secretary of State for Ireland. Obiit. 1702. Ancestor of Viscount Southwell.

own coasts and all agree in England to oppose the French, and all is well there, the King sent away his baggage this morning, he will be with you in Dublin on Munday and goeth for England in a few dayes, Count Solms is left generall, Gen. Douglass forces and ours meet att goldn Bridge neare Cashel, we expect that this night or to Morrow morning Dunganon will be surrendred, we march to Clonmel we know no more, I wrote this night to Coll. Venner since I went to the Treasury office Mr. Cunisby had not sent the order for the money therefore I did not leave the office before I saw Mr. Robinson write a letter to Mr. Fauns to supply the Coll. with a hunder pound on Demand, Mr. Fauns will be found att the Custom house Cork house I have told Dr. Hutton that you can deliver to him his gold, the Key of my Closet where it is and all my other Keys are in the blak Cabinet in the outer room, If you cannot easily find the box brake it open accomodat Dr. Hutton as well as you can, we expect that Youghall and Cork will soon surrender they will not lett me write any more.

"I am Sir

"Your most humble and

"Faithfull servant

"DUN.

"Carik 26 July 1690.

"Dr. Hutton's cloke is hanging behind my bed.

"To the reverend

"Doctor William King

"att his lodgings near the the Dolphin

"in Skinner Row

"Dublin."

"Reverend Sir,—This morning I spoke with Sr. Robt. Suthwell he assured me he would serve you before any man in Ireland and expresed himselfe with all the kindness imaginable you may rely on him^a be as kind as you can to Dr. Hutton I must follow the army

"I am Sir

"Your most humble and faithfull servant

"DUN.

"Carik, 27 July.

"To the reverend

"Dr. William King

"att his lodgings in Skinner row

"neare the signe of the Dolphin in

"Dublin."

"Reverend Sir,—Before I received your letter and our Landslords the

^a From this it is plain that Dun had strong influence with Government, and used it on behalf of his friend King, whose promotion followed very soon after this date.

best of the horses were gone, theye were sold the daye I came to Waterford, I could not trust to my own skill I tooke Mr. Persivall and Mr. Hogshaw's advice, they could not please themselves, I gave to one coming to Dublin a note of Direction to come to your lodging, he hath a baye horse to sell; I gave another letter to one Thomas Johnston who hath a Nag and a small Mare, this is by Dr. Hutton's servant who hath a horse or two of his Masters to sell, you maye give more credit to him and Mr. Johnstone then to the first, Dr. Le Can is coming from the Hospitall neare Dublin to relieve me I hope to be with you in a few days after his arrival he cometh by sea to Waterford, I praye you doe me the favor to send your servant to look out a stable for a night or 2 for 5 horses till I gett time to send them to grass We are dayly expecting to heare of Ld. Malburgh landing some where in Munster, greate guns and ammonition, pikaxes, spads, shovels, granadoes are loaded ready to saile by his order, it is supposed the design is against Cork and Kinsale the army is neare golden bridge but many are gone into Winter quarters, we have a garrison still at Castle Connell within 4 Miles of Lymrick, att Carik in Lish within 5 miles and att all the places our army was possest of neare Lymrick, I give my humble service to Mr. and Mrs. King and their Daughter

"I am Sir

"Your most humble servant

"Waterford 16 Sepr. 1690.

"DUN.

"To the reverend

"Dr. William King

"att his lodging in Skinner row

"neare the signe of the Dolphine

"in Dublin."

HIS PARLIAMENTARY CAREER.

Having succeeded in his profession, and feeling ambitious of public honours, Dr. Dun decided on entering Parliament; and, accordingly, he became a member of the Irish House of Commons in 1692. On the 27th of September in that year he was returned member for the Manor of Mullingar; and on the 29th of the same month he was also returned for the borough of Killyleagh, Co. Down. He elected to sit for the latter* on the 23rd August, 1695. He was a second time returned for Mullingar, for which manor he was for the third time returned on the 1st September, 1703.

He does not appear to have done much in the House, for the only entry which I can find respecting him, in his legislative capacity, is that on the 1st June, 1709, he was chairman of the committee which was appointed to report on the petition of John Hunt, who sought for a reward for his

* Journals of the Irish House of Commons, 10th October, 1692 (Vol. ii., p. 11).

discovery of a method of refining foul and muddy malt drink.^a The fact of his double return as M.P. says much for his popularity; and it may be remarked that at this time it was not rare for medical men to sit in Parliament, for, in the Parliament of 1695, beside Dun's name, I find those of two others, namely, Christopher Irwin, M.D., who sat for the County Fermanagh, and Thomas Molyneaux,^b M.D., who sat for the borough of Ratoath, County Meath. There is another entry respecting Dun among the Irish Parliamentary Petitions. From this it appears that Arthur, Earl of Granard, owed Dun money, for which he paid ten per cent. interest; and respecting the debt, the latter petitioned the House of Commons, on the 24th of July, 1707, to have a charge put on Lord Granard's estate. The Earl presented a petition to a like effect on the same day.

HIS CONNEXION WITH THE COLL. OF PHYS. CONTINUED.

THE CHARTER OF 1692.

Dr. Stearne, the first President of the College of Physicians, died on the 18th November, 1669, and his place does not appear to have been soon filled up. On the 27th January, 1679, Dr. Margetson^c and Dr. Howard, on the part of the Physicians, notified to the Provost of T.C.D. that the Presidentship was vacant; and in accordance with the terms of the Physicians Charter (that of Charles II.), the Board of T.C.D. proceeded to elect, and appointed Sir Abraham Yarner, Knt., M.D., to the vacant office.^d

Sir Abraham Yarner was probably re-elected in 1672 and 1678. In 1674 he was succeeded by Dr. Ralph Howard.^e The chair was filled in

^a App. to Journals, p. cciii.

^b Afterwards Sir T. Molyneux, Bart. (a younger brother to the celebrated philosopher, William); born in Dublin; educated at T. C. D., at Leyden, and at Paris; M.D. Dubl. 1687; afterwards Rega. Prof. of Phys.; Phys. to the State, and Phys.-Gen. to the Army; F.R.S. He was one of the Fellows of the Col. of Phys. nominated in the Charter of 1692; in 1798 he resigned his Fellowship, and was chosen Hon. Fell.; he was created a Baronet in 1780, and died 19th Oct., 1783. In "Harris's Ware," (which see for his life and writings), it is stated that he "practised in his faculty with great reputation till his death."

^c Thomas Margetson, one of the Fellows nominated in the Charter of 1667. For an account of him see "Monk's Roll of R. Coll. Phys." London, Vol. i.

^d Barrett's MS., p. 71—the form of the instrument in this case is given by Dr. Barrett. See, also, Register of Coll. Phys., 1866, p. 101. Sir A. Yarner's daughter, Jane, was married at St. Michan's, on 4th Aug., 1663, to Sir John Temple, and from them was directly descended the late Lord Palmerston.

^e Ralph Howard, of Dublin, M.D., made his will 6th January, 1709 (proved 8th Oct., 1710). In it he names his sons, Hugh, Robert, Bishop of Killalla (afterwards of Elphin), and William, of Dublin; and his daughters, Dorothy, wife of Anthony Dopping, and Catharine, wife of Dr. Thomas Molyneux. The Bishop's eldest son, Ralph Howard, the first Viscount Wicklow, was grandfather of the present Earl of Wicklow.—(Information from Sir B. Burke.) Ralph Howard is the Dr. Howard above mentioned. He became M.D. Dubl.; and a Fell. of Col. of Phys. in 1667; Regs. Prof. of Phys. in 1674; and died in 1710.

1675 and 1676 by Dr. Charles Willoughby ; in 1677, 78, 79, and 80, by Dr. Robert Waller ;^a and on the 24th June, 1680, Dr. Patrick Dun was first chosen President, as already noted, and he was probably re-elected annually up to 1687, when a controversy took place between the College of Physicians and the Provost and Senior Fellows of T.C.D. This controversy affected the independence of the former body, and it doubtless led to the subsequent successful application for the new Charter, which, by Dun's influence, was granted by William and Mary. In Barrett's MSS. (p. 71) this affair is thus quaintly described :—

“ Oct. 26, 1687. Dr. Connor and Dr. Dunn came to the College to signify that the College of Physicians had chosen Dr. Crosby for their President, and did desire the Provost and Senr. Fellows to confirm their election. The answer that was given to them was to this purpose: that upon restoring Trinity Hall to Trin. Coll. in the [year] 1680, there were articles drawn up and agreed upon between Trin. Coll. and the College of Physicians, and that both Colleges did enter into bonds of £300 each to the other for the performance of the said articles. And^{*} that one of the articles required that the Register of the College of Physicians should be one of those that should signify the election of the Prov. and Senr. Fellows. Another was that Trinity College did oblige themselves to confirm the election of the College Physicians, provided the person elected were a Protestant of the Church of Ireland, and that seeing their Register was not there, and that the person whom they had elected was not a Protestant of the Church of Ireland the Provost and Senr. Fellows did not think it safe nor proper for them to confirm the election of the said Dr. Crosby.”

On the 11th Nov., in the same year, the College of Physicians proposed to cancel the articles of agreement made in 1680, and the bonds relating to this agreement. To this the Board replied that “ If the Coll. of Physicians will deliver up all the writings that relate to Trin. Hall which are in their custody, and also give a release of all former grants and deeds made by Trin. Coll. to the Coll. of Physicians, concerning the said hall, that then Trin. Coll. would consent that the aforesaid articles and bonds should be cancelled. Trin. Coll. further proposes to set a lease of Trin. Hall for 40 years to the Coll. of Physicians on such terms as shall be agreed on.”—Barrett's MSS., p. 72. On the 19th May, 1688, the College of Physicians again requested the confirmation of Dr. Crosby's election, and were again refused on the former grounds.

The College of Physicians seem to have resolutely adhered to their choice in Dr. Crosby's case, and to have been supported in their conduct

^a One of the Fellows nominated in the Charter of 1667. He was also a Fellow of the London College ; M.D. of Leyden ; incorporated at Cambridge 1652 ; and admitted *ad eundem* at Dublin, 1664. See “ Munk's Roll,” *supr. cit.*

by Dun himself. About this time we lose all traces of Dr. Crosby, who, probably either died or left the country, like so many other Irishmen at this critical period.

Dun having been an influential supporter of this college was, on St. Luke's Day, 1690, chosen to succeed his non-confirmed predecessor; and his election was at once confirmed, as appears by the following entry in Barrett's MSS. (p. 72):—

“Oct. 18, 1690. An instrument was sealed and signed by the Register to constitute Dr. Dun President of the Coll. of Physicians for the year ensuing.”

Dun was probably re-elected in 1691 and 1692; and during those years, seeing that unless independent in the choice of their president the college could not work well, and that the powers conferred by the charter of Charles II. were wholly insufficient to check the practice of quackery and empiricism; he induced the college to petition Lord Viscount Sydney, then Lord Lieutenant, for a new charter similar to that of the London college. In this petition were also included requests for one of the forfeited houses, to be used as a college hall, and five or six acres of land near Dublin for a physic garden.

The petition was referred to Sir John Temple, then Attorney-General for Ireland;^a on September 3rd, 1692, he advised granting its request; it was forwarded to their Majesties; and on the 29th of the same month Lord Sydney received a royal mandate to have the new charter issued. On the 14th of December, 1692, the original charter of Charles II. was surrendered to the Lord Chancellor by Dr. Duncan Comyng;^b and the new one was probably given on the following day. It is dated 15th December, 1692, and under it, and some subsequent acts of Parliament the college is now governed.

It is not necessary to give here a full copy of this charter, which Dun was chiefly instrumental in procuring, as it has been, twice at least, printed since 1692. The copies printed at the University Press in 1856 are now easily attainable; and this latter edition having been compared with the original, and certified to be correct, by so accurate a gentleman as Dr. Steele, who was at that time Registrar, I need give no more than a table of contents of the Charter of William and Mary, which may be useful, not only to the student of medical antiquities, but may also assist in the conducting of business at meetings of the president and fellows:—

TABLE OF CONTENTS OF THE CHARTER OF 1692.

The numbers prefixed denote the Sections.

I. Recital of the Patent of Incorporation by King Charles the Second.

^a Who married Sir A. Yarner's daughter, as before noted.

^b Chosen Fellow about 1684; and nominated in the Charter of 1693. Obiit, 1724.

II. Preamble to this grant.

III. The grant—Body politic—Perpetual succession—Capable to purchase—To grant and dispose—To sue and be sued—To have a common seal—Seal may be changed—College to consist of fourteen Fellows,^a one President, and four Censors.

IV. The first fourteen Fellows (names)—To hold office for life if not removed for cause.

V. Dr. Patrick Dun appointed first President.

VI. First Censors to continue to the Feast of St Luke.

VII. Elections to be made, how and when.

VIII. Time of election of President—President to be sworn.

IX. Election of President when a vacancy occurs.

X. President to appoint a Vice-President.

XI. Election of four Censors—Censors to be sworn.

XII. Election of Censors in case of death or removal.

XIII. How the Fellows are to be chosen—Election of Fellows.

XIV. Power to summon and admonish Censors and Fellows—Power to remove or expel Censors or Fellows.

XV. Each Fellow to have but one vote—The President, or Vice-President, to have a casting vote when voices even.

XVI. Officers to be sworn to faithful execution of their duties—Oath and declaration.

XVII. Persons by whom oaths to be administered to first President, Censors, and Fellows.

XVIII. The Censors, or any two of them, to swear in future Presidents—The President, or Vice-President, to swear in Fellows and all officers thereafter chosen.

XIX. College to have a Common Hall—President to call a Court—Court to consist of not less than five Fellows, of whom the President, or Vice-President, shall always be one—Shall have power to treat of matters—To make laws and orders—To inflict punishment upon offenders by fines or imprisonments.

XX. No person to practise physic in Dublin, or within a circuit of seven miles thereof, but those licensed by the President and Fellows,^b under a penalty of ten pounds for every month—Power to sue for the same.

XXI. Power given to the President, or Vice-President, and two

^a By Statute 1 Geo. III. cap. 14, sec. 1 (made perpetual by 30 Geo. III. (1790) cap. 45, sec. 2), commonly known as "Lucas's Act;" this limitation as to numbers was removed.

^b This restriction was removed by statute 21 and 22 Vic. cap. 90 (the Medical Act), which empowers every registered practitioner to practise in any part of Her Majesty's dominions.

Censors, to supervise, examine, and punish practisers of physic, apothecaries, druggists,^a &c., &c., within the aforesaid limits.

XXII. Power given to the President, or Vice-President, and two Censors, to summon and examine all practisers offending within the limits—If they refuse to appear, or refuse to answer or be examined—To fine them for not appearing, or refusal to answer, a sum not exceeding forty shillings—Also power to fine or imprison for giving unwholesome physic; the fine not to exceed ten pounds, nor the imprisonment fourteen days.

XXIII. Power to summon persons by precept, under hand and seal, and to examine witnesses on oath, touching their knowledge of said offenders—Penalty for non-appearance or refusal to answer questions, twenty shillings.

XXIV.—Three of the Censors shall have power to enter the house, shop, &c., of any apothecary, druggist, &c., and search for, view, try, and examine all medicines, wares, drugs, &c., there kept for sale; to examine upon oath concerning the same; and to destroy all defective or corrupted medicines^b—Penalty for opposing search, forty shillings.

XXV. President, or Vice-President and two Censors, may summon before them any apothecary or druggist in Dublin, or within seven miles thereof—Penalty for non-appearance not to exceed twenty shillings—Penalty for improper compounding not to exceed three pounds—May imprison until fine be paid.

XXVI. All persons desiring to practise physic in the country, without the aforesaid limits, shall offer themselves for examination to the President, or Vice-President and Fellows.^c

XXVII. Power to the President, or Vice-President and three Censors, to summon and examine all practisers of physic without the limits aforesaid—To grant license to those qualified, and to reject those not duly qualified for the same.^d

XXVIII. None (excepting graduates in the Universities of Cambridge, Oxford, or Dublin) to practise physic outside the limits aforesaid, unless licensed, under a penalty of five pounds per month.^e

XXIX. Plaintiffs shall have their costs of suits brought to recover the above-mentioned penalties. *Provided always* that graduates in physic of the University of Dublin, having performed their full acts, be admitted

^a So far as apothecaries, druggists, &c., are concerned, this power was enlarged and confirmed by 9 Geo. II. cap. 10, which, after several re-enactments, was transformed into Lucas's Act, still in force. For the provisions of this, see Register K. and Q.C., P. 1866, p. 11.

^b Confirmed by Lucas's Act.—See Note to Clause XXI.

^c See Note to Clause XX.

^d See Note to Clause XX.

^e See Note to Clause XX.

into the College, without further examination, on payment of the usual fees.^a

XXX. Power to examine and license midwives, and to punish those not licensed—All fines, penalties, &c., to be approved by a Court, and registered, before being levied—Appeals for relief from fines or penalties to be made within one month to the persons hereafter nominated.

XXXI. Visitors constituted.^b

XXXII. Power of Visitors to hear and adjudge appeals—May remit causes back again to the President and Fellows.

XXXIII. All judgments made by the Visitors to stand good, without further appeal.

XXXIV. In case appeals be not prosecuted within six months, the President and Fellows may proceed in every such cause, sentence or decree, as if no appeal had been made.

XXXV. The President and Fellows to proceed on judgment on appeal (after remitted), as in other cases before.

XXXVI. Offenders to be questioned or punished within one year after the offence.

XXXVII. All fines given to the President and Fellows, with power to sue for and levy same.

XXXVIII. Fines to be distributed amongst the poor of the parishes or places where the offences were committed. The President and Fellows to pay six pounds, yearly rent, to the Crown.

XXXIX. Power to appoint one of the Fellows as Registrar. His duties. Other officers may be appointed. Registrar and other officers to be sworn.

XL. Power to take recognizances of offenders, and to commit to prison persons refusing to enter into recognizances.

XLI. Gaolers to obey the warrants of the President and Censors, and safely keep all persons committed in their prisons. Penalty for doing the contrary, double the fine for which the person shall be so committed to their charge. Provided such fine be not above twenty pounds: one half to go to the Crown, and the other to the President and Fellows, for the use of the poor, as aforesaid.

XLII. Liberty to take annually for dissection the bodies of six persons executed for crime, in the city or county of Dublin. Provided that after such use they be decently buried at the costs and charges of the said President and Fellows.

XLIII. Liberty to purchase lands, of a clear yearly value not exceeding two hundred pounds sterling.

XLIV. Members of the College to be exempt from serving on juries ;

^a Repealed by 40 Geo. III., cap. 84, sec. 85 (School of Pysic Act).

^b Confirmed by Statute 40 Geo. III., cap. 84, secs. 38-40 ; which also confirmed clauses XXXII. and XXXIII.

from being chosen as churchwardens, constables, &c.; from all watch and ward; and from bearing and providing arms, within the city of Dublin or within seven miles thereof.*

XLV. Every Apothecary in Dublin or within seven miles thereof, shall before taking an apprentice bring him before the President and Censors to be examined touching his knowledge of the Latin tongue. Penalty for infraction, not to exceed twenty pounds sterling.

XLVI. These Letters Patent to be good, firm, and effectual in the law.

XLVII. If enrolled within six months, in the Rolls of the High Court of Chancery in Ireland.—15th of December, in the fourth year of our reign.

Witness.

DOMVILE.

WHAT BECAME OF TRINITY HALL.

The grant of the new charter to the College of Physicians was probably made with the consent of the Board of T.C.D.; for their privilege of electing the President seems to have been given up without any protest on their part. For this reason I am inclined to think that the Board received the *quid pro quo*, in the shape of the College of Physicians yielding to the demands contained in the proposal made by the Board in 1687, to cancel the agreement of 1680. (See Barrett's MSS., *supr. cit.*, p. 72.) If such occurred, as is more than probable, it may be assumed that Trinity Hall reverted to the Board in or about 1693, and nearly all that is subsequently known of it, I have detailed in my memoir of Dr. Stearne (p. 28), before referred to. It will be observed that in the proposal to cancel the agreement it is distinctly stated that all the writings relating to Trinity Hall should be delivered up to the Board of T.C.D.; and this gives a probable solution as to what became of various documents which the College of Physicians must have had while occupying Trinity Hall. The only two books of that period now in possession of the College have been referred to and quoted from already;^b and they contain internal evidence of the existence of minute books, which are now lost. It appears that during the provostship of Dr. Baldwin many valuable papers belonging to T.C.D., extracts from which Dr. Barrett copied, were lost; and no account can be got of them. I may say here in passing, that from one of the books just noted it appears that Mrs. Stearne, widow of the first President, occupied Trinity Hall, as a residence, for several years after her husband's decease; and paid the College of Physicians rent for her lodgings.^c

* Confirmed by the Medical Act, and extended to registered practitioners everywhere.

^b D'Olins' book, described on p. 28 of my memoir of Stearne; and "No. 7 Old Accounts," *supr. cit.*

^c In the Provost's house there is, among the heir-loom portraits, one of a Doctor of Physic, name unknown. I believe it to be a portrait of Dr. Stearne.

The College of Physicians must have given up Trinity Hall on the surrender of the old charter, for Dr. Barrett records the appointment of a tutor in arts as master there on the 9th of July, 1694.^a From this it appears that in 1694 it was given up to the purposes of general education. The next entry respecting it in Dr. Barrett's MSS. is "Nov. 21, 1694. Lease ordered of Trin. Hall for 41 years to Math. Shaw;" and his last notice of it is: "24th Jan. 1710, two leases to Dr. Jn. Barton, of Trin. Hall perfected for 40 years."

Among other evidence laid before a committee of the House of Commons on the 16th December, 1783, on the School of Physic question, Dr. Dabzac,^b who was a Senior Fellow of Trinity College at that time, deposed that "Trinity Hall reverted to Senior Fellows, and is now [1783] in the possession of ye University." In *Gilbert's History of Dublin*, (iii. 17), it is stated that "a portion of its site is occupied by the Alms House of St. Andrew's Parish."

THE COLLEGE BEGINS TO MEET AT DUN'S HOUSE.

The first meeting of the college under the new Charter took place on the 7th January, 1694^c; and it probably was held in Dr. Dun's house on the Inn's-quay.^c From the minutes of this period it seems that the subject of our memoir was very energetic in claiming privileges under the new Charter; for, under date of Feb. 18th, 1694^d, it is recorded that he and the fellows demanded from the Sheriff and obtained the body of a malefactor executed the same day; and Dr. Gwythers,^d one of the fellows, was appointed to dissect it. Shortly after this (March 1st) Dun had a quarrel with Dr. Ralph Howard, another fellow, and it is stated that they submitted to the decision of their brethren in the matter; for, "the censure of the college being read to them they accordingly allowed of and complied with it, and are now in presence of the college returned to their former friendship." On the same day it was recorded that the body of the criminal had been dissected, and duly interred.^e

HIS MARRIAGE, AND HIS SON BOYLE.

On the 11th of December, 1694, Dr. Dun took the important step of

^a "Sir Smith-chosen Master of the School in Trinity Hall."—Barrett, MSS., p. 74.

^b Henry Dabzac elected Fellow 1760; co-opted vice John Stokes, 1775. Died 12 May, 1790.—Dub. Univ. Calendar.

^c This may be inferred from the College Journal.

^d Charles Guither or Gwythers, M.D., Dubl., 1688, one of the Fellows nominated in the Charter of 1692; he died in 1700.

"Dr. Gwythers, a Physician and Fellow (probably of Coll. of Physicians), brought over Frogs in 1692 from England, which were first in the ditches of the Coll. Park. See Swift's Works, note Tatler, Vol. iv., page 206." Barrett MS. Miscella. Papers, No. 7.

^e College Journal.

getting married. By this act he became connected with some of the best and most influential families in this country.

The lady of his choice, who figured largely at a later date in the history of the College of Physicians, was Mary daughter of Colonel John Jephson, third son of Sir John Jephson, of Mallow, and of Bridget, daughter of Richard Boyle, Archbishop of Tuam, and sister of Michael Boyle, Archbishop of Armagh. Mrs. Dun's eldest brother, William Jephson, was Dean of Lismore; her second brother, Michael, became Dean of St. Patrick's, Dublin; her third brother was named John; she herself was the fourth child of her parents; the fifth was a sister, Elizabeth, who married—firstly, William Becket, Esq.; and secondly, Sir John Topham, Knight; and the sixth was also a sister, Katherine, who was wife of Colonel William Richardson, of Rich Hill, in Armagh.

Patrick Dun and Mary Jephson had issue one son, Boyle, who was baptised on the 24th of November, 1697, and who died young. The exact date of his decease I have not been able to ascertain. The fact that Dun had a child at all seems to have escaped notice hitherto.

I am indebted to the great kindness of Sir Bernard Burke for the following documents, about which he took great trouble; and, I may here express my thanks for his invariable and courteous attention to any request having for its object the advancement of medical literature. He gave me the fullest means of gaining all the information at his disposal. Appended to one of these documents are the arms of Dun, and also those of his father. The difference between them is a heraldic curiosity; and, as I lately inspected Dun's will at the offices of the Probate Court, I can vouch for the fact of the difference above referred to. The accompanying woodcuts were executed by Mr. Oldham.

ULSTER'S OFFICE, 28th July, 1866.

Sir Patrick Dunn, of Dublin, Knt., Will dat. 16th November, 1711, proved, 1713. He left his estate to establish a Professor of Physick in the College of Physicians of Dublin. Died, 24 May, 1713. Buried, 27 in St. Michan's, Dublin.

Mary, daur. of John Jephson, by Bridget Boyle, his wife, married 11th December, 1694; died 19 Jan., 1748, æt. 95; and was buried with her husbd.

Boyle Dunn,
Bapt. 24 Nov., 1697.
Died young.

J. BERNARD BURKE,

Ulster.



Arms on the Will of Sir Patrick Dun,
of Dublin.



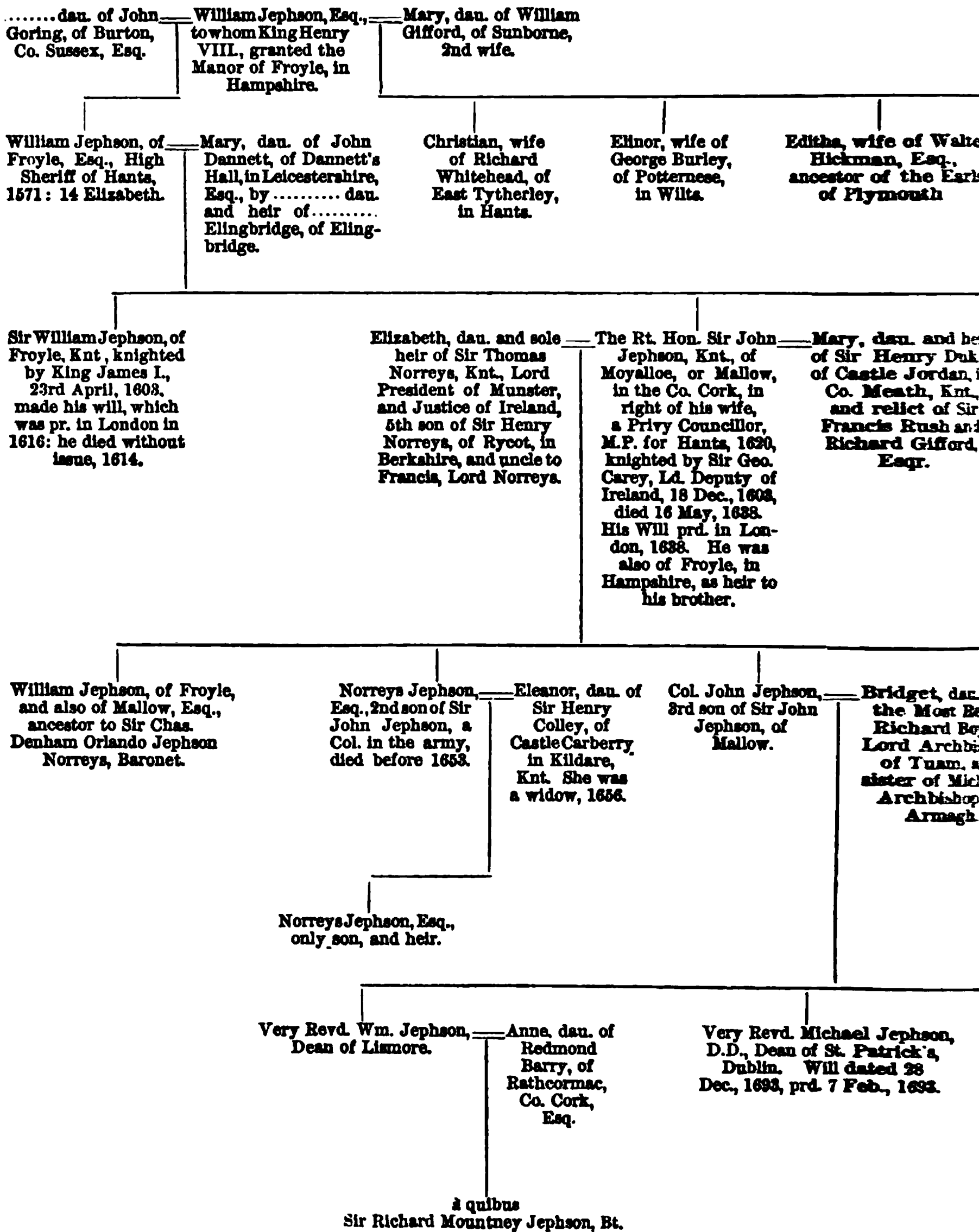
Arms Registered in Ulster's Office to
Charles Dun, of Aberdeen.

From the Seal still visible on Sir Patrick Dun's Will in the Court of Probate, Dublin, it appears that Sir Patrick bore for Crest "the Dexter Hand" simply; but Nisbet's *Scottish Heraldry*, a work of great authority, gives for the Arms of Dun, the Sword and the Padlocks; for Crest, "a Dexter Hand holding a Key *Ppr.*," with "*Mecum habita*" for motto. This latter Crest has long been in use on the Seal Stamp in Dun's Library. Possibly Sir Patrick may have used both Crests. Certain it is the Crest of a Dexter Hand (without the Key) is registered in Ulster's Office to Charles Dun of Aberdeen, the father of Sir Patrick.

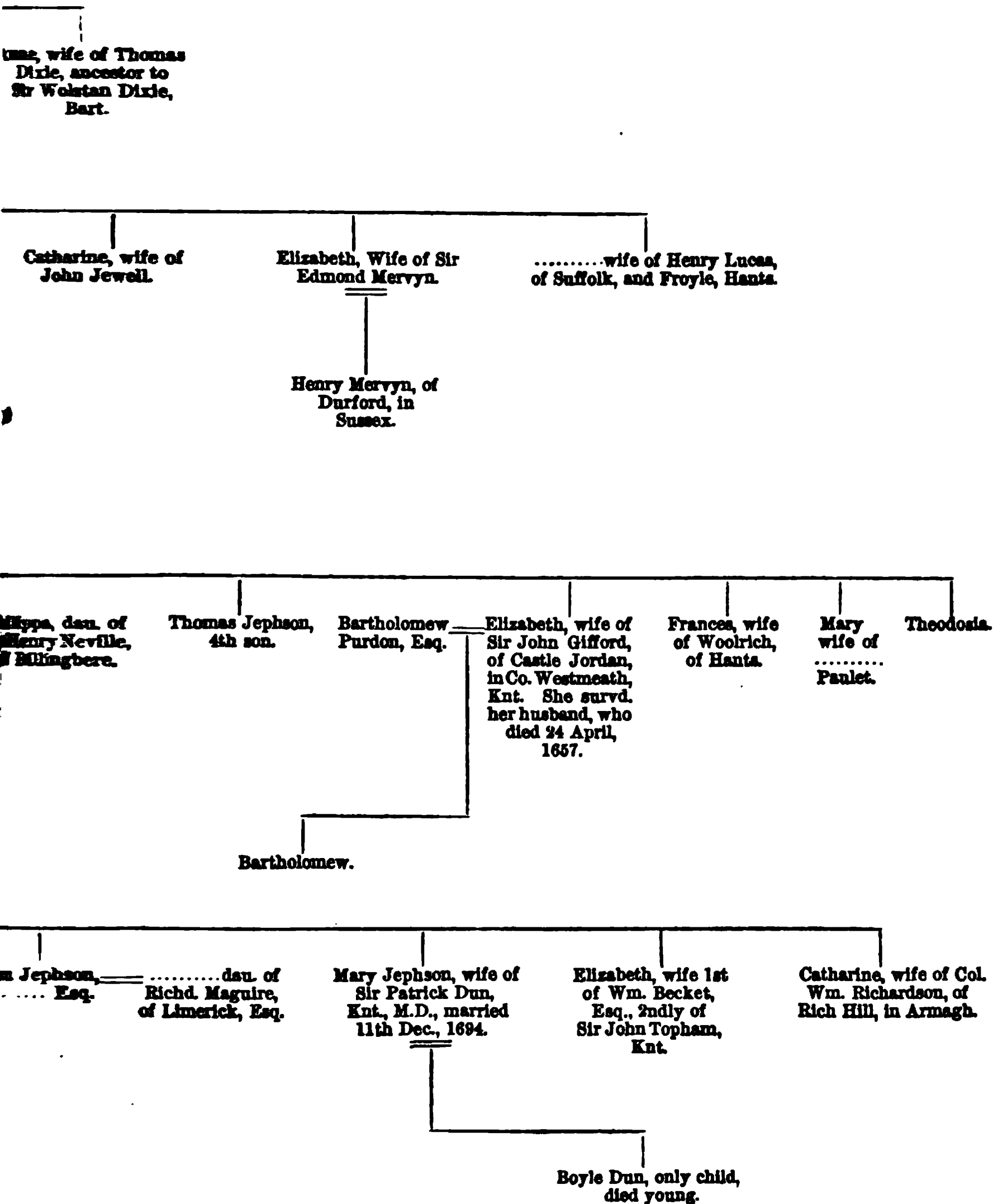
It has already appeared that Dun was engaged in every important transaction concerning the College of Physicians. In "Sir P. Dun's Book," p. 74, is a copy of two curious documents, to one of which his name is signed. It gives some clue to the professional position of the College in 1695, to Dun's position in that body, and to the mode in which vexed questions in matrimonial causes were then conducted. It consists of a commission from Archbishop Narcissus Marsh* to the College, to examine and report on the condition of one Hugh Geoghegan, in respect of a case then pending in the Metropolitan Consistorial Court. It also contains the report of the College. For obvious reasons I give both documents in the original Latin:—

* Born in Wiltshire, in 1638; Principal of St. Alban's Hall, Oxon., 1678; Provost T.C.D., 1678; Bishop of Leighlin and Ferns, 1683; translated thence to Cashel, 1690; thence to Dublin, 1694; and finally to Armagh in 1702. He died 2nd Nov., 1718, *stat.* 76; and was buried in the churchyard of St. Patrick's Cathedral, Dublin, adjoining the library which he built and endowed, and which is known by his name to the present day. His monument is now in the Cathedral. See his life and writings in "*Harris's Ware.*"

LADY DUN



PEDIGREE.



ARCHBISHOP NARCISSUS MARSH'S COMMISSION TO THE COLLEGE OF
PHYSICIANS, IN RE GEOGHEGAN.

NARCISSUS providentia Divina Dubliniensis Archiepiscopus Hiberniæque Primas et Metropolitanus. Dilectis nobis in Christo Honorabili et egregio viro Radulpho Howard præsidi, et Sociis Collegii Regis et Reginae Medicorum Dublin in Hibernia SALUTEM in Domino sempiternam QUUM honorabilis et egregius vir JOHANNES TOPHAM miles Legum Dr. vicarius in Spiritualibus generalis Curiaeque Consistorialis Metropoliticae Dublin officialis principalis legitime fultus, rite et legitime procedens in quadam causa nullitatis matrimonii inter Claram Hussey alias Geoghegan reputatam partem actricem seu querelantem ex una, et Hugonem Geoghegan reum seu querelantum partibus ex altera ratione frigiditatis seu impotentiae prefati Hugonis ad Coitum, varios actus expedivit usque ad informationem in eadem causa, Nos ad plenius cognoscendum et probandum dictam frigiditatem seu impotentiam prefati Hugonis ad Coitum, Corpus seu membrum virile ejusdem Medicorum Judicio inspiciendum scrutandum si sic lubet parti prefati Hugonis, modo et forma infra scriptis indulsumus VOBIS igitur in ea parte scribendum fore ad petitionem procuratoris prefati Hugonis decrevimus (Justitia mediante) SCIATIS igitur quod nos de vestra fidelitate, Circumspectione et Judicio in arte quam profitemini plurimum in Domino confidentes, vobis vel aliquibus quinque vestrum plenam potestatem et auctoritatem damus concedimus, et committimus, ad Corpus membrumque virile prefati Hugonis inspiciendum scrutandum necnon super Interrogatoriis omnibus que aliis viis mediis ac modis seu Circumstantiis quibus melius scire poteritis de frigiditate seu Impotentia prefati Hugonis ad Coitum examinandum congruis et opportunis loco et tempore, per vos vel aliquos quinque vestrum elegendis, et assignamus prefatum Hugonem ad se Corpusque ac membrum suum virile amandandum exponendum coram vobis vel aliquibus quinque vestrum ad subeundum scrutinium inspectionem et examinationem predictam et insuper rogamus quod circa omnia et premissa singula vos vel aliqui quinque vestrum intendatis et ea faciatis et exequamini indilatè cum effectum ita quod quicquid in premissis vos vel aliqui quinque vestrum feceritis nobis aliove Judici dictae Curiae Consistorialis Metropoliticae Dublin. predictae cuicunque in hac parte competenti immediate post executionem presentium debite certificetis clausim et authentice sigillatorum, una cum presentibus Datum undecimo die mensis Januarii 1695.

REPLY OF THE COLLEGE TO THE FOREGOING COMMISSION.

Reverendissimo in Christo Pater, Nos quorum nomina infra scripta sunt ; Præses et Socii Collegii Medicorum Regis et Reginae in Hibernia, literis vestris undecimo die Januarii 1695 ad nos datis fideliter obsequentes ; Hugonem Geoghegan examinavimus, et ejus partes Generationi inser-

vientes inspeximus, ut nobis innotesceret, num ad Coitum Potens, vel ad actus venereos exercendos plane Inhabilis esset Sed cum ad pleniorē rei certitudinem necessarium duximus ut accuratiori scrutinio sese subjiceret, ac ut magis fida sed honesta suæ virilitatis judicia nobis palam faceret; Prædictus Hugo Geoghegan plane renuit, et vel non Potuit, vel noluit a nobis satis casta requisita peragere: adeo ut Mandatis vestris plene satisfacere hac in re non licuit. nihilominus examen hoc nostrum licet mancum Reverentiæ vestræ notum facere tanquam debiti nostri officii testimonium volebamus, Datum 20 Februarii 1695.

RAD. HOWARD, Præs.

PAT. DUN, Censor.

J. MADDEN, Censor.

D. CUMYNG.

THO. MOLYNEUX.

DUN'S KNIGHTHOOD, &c.

On the 29th of January, 1696, Dr. Dun was knighted by Charles, Earl of Mountrath, and Henry, Earl of Drogheda, Lords Justices; and the next fact which I can find regarding him is, that, having in 1704 represented that there was an hospital in Dublin for the sick and infirm of the army, and that no physician had been appointed to attend them since the death of Queen Anne, he was appointed on the 25th of March, 1705, "Physician General of the Army," with a salary of ten shillings per diem.^a

He appears to have been a regular attendant at the College meetings to the last; he was engaged in all its public, and in its more important private transactions throughout; as already noted, he filled its principal offices many times, and, according to the testimony of the College journals, he discharged all his duties to that body with industrious and energetic fidelity.

His son Boyle being probably dead, he made his celebrated will, and his equally celebrated deed concerning a professor of physic, in 1711. I give the *ipsissima verba* of both documents.

THE WILL.^b

"Sr. Patrick Dun's Last will and Testament.

"IN THE NAME OF GOD AMEN The sixteenth Day of November and in the Year of our Lord God one thousand seven hundred and eleven and in the tenth year of the Reign of our Sovering Lady Queen Anne I

^a Dr. A. Smith—Letter to Dr. Geo. Alex. Kennedy, in "Kennedy's Index to the College Journals."

^b This Will, and the Deed which follows it, are copied from "Sir P. Dun's Book," a MS. in the archives of the Coll. of Phys., and described in my Memoir of Dr. Stearne. I may here thank Mr. Fennell, of Dun's Library, for making accurate copies of these documents, and of several others, for the purposes of this memoir.

Sr. Patrick Dun Knight being somewhat indisposed in Body but of Good and perfect memory (thanks be to Almighty God) Do make this my Last Will and testament in manner and form Following that is to say,

“FIRST I resigne my Soul into the hands of my Creator from whom I received it, in full hope of eternall Salvation thro: the merits of my Lord and Saviour Jesus Christ and I desire my Body may be privately Interred without any Publick Noise or Ostentation in a Vault Adjoyning to the South West wall in the Inside of the Parish Church of new St. Michans in Dublin, which Vault I purchas'd from the Minister and Church Wardens of the sd. Church for me and my heirs for ever, by Deed Indented and duly perfected by them, Wherein is a Clause Excluding any Person from being buried therein without leave first obtained from me my Heirs, or Assigns. •

“AND as to my Worldly Estate I will that all my Debts which I may owe at the time of my Death, be Justly Paid within a reasonable time.

“AND WHEREAS by Articles of Agreement made upon my Intermariage with my now Dear Wife bearing date the tenth day of December one thousand Six hundred Ninety and Four, and made or mentioned to be made by me of the first part, my Sd. Wife and Bridget Jepson her Mother of the second part, and Sr. John Topham of the City of Dublin Knight, Francis Cuffe of the same Esqr. William then Lord Bishop of Derry and now Lord Arch-Bishop of Dublin, and Robert King of the City of Dublin Gent: of the third part: they my Sd. Wife and her Sd. Mother, did grant and Assigne over the sum of Nine hundred Pounds ster: in ready Money and Money out at Interest on severll. Security's Therein particularly mentioned being the Marriage Portion of the Sd. Mary unto the Sd. Sr. John Topham and other the Sd. Trustees in the Sd. Articles Mentioned, and I did also thereby Assigne over the sum of two thousand pounds Ster: in Money out at Interest on Severll. securitys therein particularly Mentioned and in ready money unto the Sd. Trustees both wch. Sd. Sums making together two thousand Nine hundred pounds, were so Assigned in trust that the Sd. Sr. John Topham and the other trustees should as soon as conveniently they could lay out the same in the purchase of Lands and Tenements to be settled on me for my life, and after my Death to my wife for her life for her Joynture, in case I should have no Issue by her living at my Death, in full Satisfaction of all Dower and thirds, she might claim with remainders to our Issue Male, and for want of Such to our Issue female, and for want of such Issue to my own right heirs, and in trust that untill such a Purchase Could be had the ready money should be laid out at Interest, And that the money that was then out at Interest should be continued out at Interest, or as the same should be paid in, it should be again put out to Interest in the Name of the Sd. Trustees to the intent and in trust that I should Receive the Interest during my life, and in case I should Die without having Issue

by my Sd. wife living at my Death then my Wife should have the Interest thereof for her life, and in case of my having Issue living by her at my Death, then she to have two thirds, and the other third to goe to such Issue, and after her Death the Interest of the Sd. whole sum to goe to our Issue and for want of such Issue to my Execrs. Admrs. and Assigns, as by the Sd. Articles among other things therein contained may more fully and at large appear.

“AND WHEREAS that part of the Sd. two thousand nine hundred pounds which Consisted of ready money was put into or continued in my hands, and all the Sevrll. Sums of money which are Mentioned in the Said Articles to have been then out at Interest have been since paid in unto me or become desperate Except the sum of one hundred pounds therein mentioned to be due from Mr. Arthur Emerson and Mr. Anthony Percy.

“AND WHEREAS no real Estate was purchased by the Said Trustees in pursuance of the Sd. recited Articles and whereas I have by the approbation and consent of my said Wife, and of his Grace the now Lord Archbishop of Dublin and of the Sd. Mr. Robert King the then surviving Trustees Mentioned in the Sd. Articles lent unto Gerard Fitzgerald of Rathroane in the County of Meath Esqr. the sum of Eight hundred pounds Ster. Principal money which I paid unto Stephen Ludlow Esqr. who had a Mortagage from the said Gerard Fitzgerald for the Sd. Sum, for Security of which Sd. sum of Eight hundred pounds, the Sd. Stephen Ludlow and the said Gerald Fitzgerald have by deeds of Lease and release dated the Eighth and ninth Days of June one thousand Seven hundred and five Mortgaged to me the town and Lands of Edmondstown als. Bally Edmond Moyle Aghadowe and of severall other Towns and Lands lying and being in the County of Meath, Westmeath and Cavan therein particularly mentioned, as by the Said Deeds may among other things more fully & at large appr.

“AND WHEREAS I have Likewise by the Approbation and consent of my Sd. Wife and of the Sd. Surviving Trustees laid out at Interest the sum of two thousand pounds Ster. upon a Mortgage made to me for the Sd. sum and the Interest thereof by Nicholas Colethirst of Ballyaly in the County of Cork Esqr. and Elizabeth Bromby his Mother by Deeds of Lease and release dated the Sixth & Seventh Days of February one thousand seven hundred and Eight, of the Town and Lands of Magulleen Ballyshoneen Knocknemara and sevll. other Lands therein particularly Mentioned Lying and being in the County of Cork

“Now my Will is and I do devise that the Sd. Sevll. Sums of money by me lent on the Sd. Securities shall be Deemed and looked upon as put out at Interest in pursuance of the said Marriage Articles and that my Sd. Wife shall have and receive the Interest of the Sd. Sevll. Sums of Eight hundred pounds lent to the Sd. Fitzgerald herein before mentioned and of the two thousand pounds lent the Said Mr. Colethirst and Mrs Bromby

wch. shall grow due from the time of my Death for and during her life, the same together with the Interest of the Sd. Sum of one hundred pounds particularly mentioned in the Sd. Articles to be due from the Sd. Mr Emerson and Mr Percy and which hath not been paid in, to be in full satisfaction and Accomplishment of the Sd. Articles and in full satisfaction and Barr of all Dower and thirds which my Sd. wife can or may Claim in, to or out of my Real and Personal Estate.

“ITEM, I will and desire my Overseers to take a just and faithfull Account of all money left by me either in my own or wifes Custody, or in the Hands of any Banker, and of all and every sum and sums of Money yt. shall or may belong to me at the time of my Death, and also take a faithfull account of All arrears of Rent and arrears of Interest money wch. shall be then Due.

“AND I do give full power to my Overseers, with the approbation of my wife to receive in all such sums of money as are already put out at Interest and shall be tendered to be paid in, and to make sufficient discharges for the same, and to lett out again all such sums of money and to put out all money that shall be had in our Custody or in the hands of any Banker to Interest on sufficient security's in the Names of my Overseers and the survivors or survivor of them his Execrs. Admrs. Without any hazard or damage to attend them in so doing and that all the Deeds and Bonds taken for securities of the Sd Sums of money do remain in the hands of my Overseers, my wife having Copies of the Same.

“ITEM, I bequeath to my Said Wife the Clear Rents and Profits of all my Real Estate together with the Interest of all such money as I shall have out at Interest at my Death or shall be put out at Interest by my Overseers pursuant to this will (over and above what is intended for her joynture) to her own proper use whilst she Continues my Widdow and no longer she paying the reserved Yearly Rent of my real Estate and performing all such other Conditions as I am Obliged to do for the same, and she being at all the necessary charges for the preservation of my Just Right and Title to the same and to the Money so out at Interest to every part and parcel thereof.

“ITEM, I give my Sd. Wife whilst she Continues my widdow the use of my Dwelling House on the Inns, Provided she keep it in good repair and pays the Rents and performs all the Covenants and Conditions which I am obliged to do by my Lease always excepting and reserving out of this Bequest one Convenient Room in the Sd. Dwelling House to be particularly set apart by my Wife with the approbation of my Overseers for the preservation of my Books.

“AND I give my Exrs. by the Consent of my Overseers and the Survivors or Survivor of them power on the Determination of any of the Leases of the aforesaid Fee-farms to lett them out on Lease again, at the best improved Rents that can be had for them for twenty-one Years in

Possession but not in reversion, on this Condition that if any Fine be taken then the Lease or Leases so made shall be Utterly Void ; The Lease or Leases so made to be always in the Name of my Exrs. and the survivor of them with consent as aforesaid And the Sd. Indentures of Leases or Lease to remain in the custody of my overseers, my Wife having a Copy of the same.

“ITEM, I give to my Sd. Wife to her own proper use a purse of Gold and all such other pieces of Gold and Silver and other Medalls which She Shall on her word affirm I Presented her with by way of gift since our Intermarriage without being accountable to any person for the same.

“ITEM, I give my Sd. Wife all such Jewells and Rings as I presented to her either before or since our marriage.

“ITEM, I give my wife all my Household Stuff and Furniture Excepting my Books and Plate.

“ITEM, I give my wife the Lawfull use of all my Plate as long as she shall continue sole and unmarried with a Liberty to alter the fashion thereof at her discretion provided always the same weight and Standard be preserved she first giving security to my other Exr. that the Sd. plate or the same weight and Standard shall be delivered to him on her Marrying again, and if she continue my Widdow at the time of her death, then I bequeath all such Plate as I shall have at my Death to her, her Exrs. Admrs. and Assigns.

“ITEM, I give my wife my Coaches and horses and all the Furniture and provisions belonging to them or laid in for them at the time of my death.

“ITEM, I give my Wife all such Books as she shall say on her word were her own before our Marriage, or were bought by her Since Marriage, or were presented to her by me or any other Person since our Marriage.

“ITEM, I give my Wife such Fire Arms as she shall Judge Convenient for the preserving her House or for her Servants to travile with, the residue I give to Dr. Patrick Mitchell, together with all such Mathematical Instruments and other curiosities as are in my Closets, or used to be kept there, excepting my Books.

“ITEM, I give and bequeath to Patrick Dunn, of Tardy,^a near Aberdeen, in North Brittain, Esqr. one Thousand pounds Ster. part of the Sd. Two Thousand pounds lent to Mr. Colethirst and Mrs. Bromby to be paid him after the Decease of my Dear Wife.

“ITEM, I Leave three hundred and thirty three Pounds Six Shillings and

^a Son of one of the nephews of the Principal of Marischal College, mentioned in his deed already quoted. That family is now extinct in the male line ; Archdeacon Bisset, of Lissendrum (I believe), claims to represent it in the female line through Annie Dun, great-niece of the Principal, who married his ancestor, Robert Bisset, about 1724.

Eight pence to and among the Children of my Eldest Sister Katherine^a Deceased.

“ITEM, I leave three hundred thirty three pounds Six Shillings and eight pence to the Children of my third sister Bessy^b who is also Deceased.

“ITEM, I leave Four hundred thirty three pounds Six Shillings and Eight pence to my sister Rachell,^c if Living at the time of my Decease, and if not, then I leave the Sd. Sum of Four hundred thirty-three pounds Six Shillings and Eight pence to her Children all the Sd. Severall sums devised to my Said Sisters Children, and my Sd. Sister if Living, to be paid after the Death of my Sd. Wife.

“ITEM, I devise all the Remains of my Real and personal Estate not above disposed of viz. my Fee farms and Real Estate after the Determination of the devise to my wife, during her widdowhood, and all my personal Estate after discharging and fullfilling the Legacys and devises herein before mentioned unto the Sd. Patrick Dun of Tardy Esqr. and unto the said Doctor Patrick Mitchell their Heirs, Exrs. Admrs. respectively to the use of and in trust for themselves their Heirs Exrs. Admrs. respectively untill there shall be a Professor of Physick Established in the Colledge of Physicians in Dublin to Continue for ever by Succession according to the Rules and Directions and under the Regulations by me laid down or to be laid down and expressed in a certain Instrument in writing under my hand and Seal Intituled, The Scheme which I intend to have observed for the Establishing a Professor of Physick in the College of Physitians in Dublin and from and after Such time as such Professor shall be accordingly Established. Then in trust that the Said Patrick Dun of Tardy and Patrick Mitchell and the Survivor of them and his Heirs and Assigns shall by such Conveyances and Methods as shall be reasonably desired by the Council of the Sd. Colledge of Physitians convey and assure or secure the Sd. residue of my Sd. Reall

^a Wife of Thomas Mitchell, mentioned in the Deed. He was father of Patrick Mitchell, M.D., one of the Trustees to the Will, and a Fellow of the College of Physicians. His election is dated 1 May, 1704.

^b Wife of Alexander Anderson, of Bourtie. Their son Patrick had a daughter, Mary, who was second wife to Provost William Young, of Aberdeen. Numerous descendants of this marriage are in existence; among them may be noted:—John Leith Ross, of Arnage in Aberdeenshire; Patrick Black, M.D., Physician to St. Bartholomew's Hospital, London; and John Abercrombie, M.D., Physician to the General Hospital, Cheltenham. Patrick Dun, of Tarty, and [Sir] Patrick were godfathers to Bessie Dun's son, Patrick, in 1693. A full account of all these descendants of Bessie Dun is contained in the private memoir of Young already quoted.

^c Wife of James Moire, of Ferrie Hill, near Aberdeen. None of these three sisters is included in the baptismal register of the Duns already quoted. Probably they were the children of Charles Dun's first marriage, or were born after 1654, to which year only search was made.

and personall Estate so to them devised, to answer and fulfill my intentions Expressed in the Sd. Paper Intituled, The Scheme which I intend to have observed for the Establishing a Professor of Physick in the Colledge of Physitians in Dublin, be it by Act of Parliament or otherwise howsoever.

“AND I nominate and appoint my wife and the Sd. Dr. Patrick Mitchell Exrs. of this my last will and Testament.

“AND LASTLY I declare and appoint my good Friends the Sd. most Reverend Father in God William Lord Arch-bishop of Dublin and William Whitshed and Joseph Deans of Dublin Esqrs. or any two of them overseers of this my Last Will and Testament, and do revoke all former Wills by me made. IN WITNESS whereof I the said Sr. Patrick Dun have subscribed this my Last Will and Testament, with my own hand and thereto put my Seale the Day and Year first before written.

“PATRICK DUN.

“Signed sealed and published in the presence of these Witnesses who also subscribed their Names as Witnesses in the presence of the Said Sr. Patrick Dun—Edward Makay, Will: Sumner, Tho: Cooke. Not: Pub:”

This will was proved 24 June, 1713.

THE DEED.

“SR. PATRICK DUN’S DEED FOR CONSTITUTING AND PROVIDEING FOR A
“PROFESSOR OF PHYSICK.

“WHEREAS in my last will I have hinted or signified That I had left it to his Grace Dr. William King present Arch-Bishop of Dublin and to my Dear wife Mary alias Jephson, to conceive and contrive a Deed for settleing and disposing of such Interest as may remain in me (after the Death of my Dear Wife) of my dwelling House in the Inns in Oxmantown Dublin and my Interest in two fee Farms in the Barony of Uppertown in the County of Waterford in Ireland I declare that it is my desire and intention to make provisions for one or two Professors of Physick to read public Lectures and make publick Anatomical dissections of the several parts of human Body’s or Body’s of other Animals, to read Lectures of Osteology Bandage and Operations of Chirurgery to read Botanic Lectures, Demonstrate Plants publickly and to read publick Lectures on Materia Medica, for the Instruction of Studients of Physick Surgery and Pharmacy And if the Society of the Inns would Consent to grant the reversion of my House after the Expiration of my lease from them for a Physic School and habitation for the foresaid Professor he being oblig’d to keep the same in good and sufficient repair or Liable to have the Rents of the Fee farms or part thereof Sequester’d for the

reparation of the same by the Guardians of the same hereafter Named I am willing to give my Title and Interest in my said House after the Death of my Dear Wife for the Said use and also for an Hall or Place for the King and Queens College of Physitians to assemble and hold therein, They Contributing according to their accommodation towards the repair of the Same. And provided that the aforesaid Professor or Professors be elected Fellow or Fellows of the Said College of Physitians in the first Vacancy that shall happen after his or their Election to the Professors Place, and Provided that the Sd. College Physicians give no unnecessary disturbance or hindrance to the peaceable dwelling of the professor or professors in the Sd. House. Likewise I would give my Books for the Lawfull use of the Sd. Professors and College of Physitians, Provided the Sd. Professor or Professors Inhabiting the Sd. House immediately after their Election and before they have Possession given them of the House by the Guardians, give Bond and security to keep and preserve the Sd. Library and all and every Book and Books in it, and if any should be lost or wanting, to pay for or purchase another of the same kind, the same paper and Edition or better in the room thereof. One of the Guardians to be his Grace the present Arch-Bishop of Dublin during his pleasure or Natural life, and after him such other person as he may be pleased to nominate, by writing under his hand and seal for that purpose before his Death without affixing or Appropriating that place to the Arch-Bishop of Dublin (if His Grace shall so think it expedient) for the time being the Present Lord Blessington and the Heirs Males of his Body lawfully Begotten.

“My Cousin-german Patrick Dun of Taertie in Scotland Near Aberdeen, and the Heirs of his body lawfully begotten The Heirs Males Lawfully begotten upon my three sisters by the late Tho. Mitchell The deceased James Moire of Ferrie-hill and by Alexander Anderson of Bourchie the Heirs Males of John Jepson Esqr. my Wifes Nephew the Heirs Males of Brother Will. Jepson and of her Brother Dean Reader lawfully begotten upon her sister Elisia.

“The Examinators to be the Provost for the time being of Trinity Coll : The Professors of Physick in the same the Presidt. for the time being of the King and Queens College of Physicians in Ireland the two Eldest Censors Yearly Chosen by the Sd. College The Arch-Bishop or any other Person may examine, but the forenamed Examinators to be first duly Sworn or to take a voluntary Oath to Examine faithfully without favour affection or prejudice to any Candidate and to deliver their oppinion of such person as they shall Judge Qualified.

“THAT upon a Vacancy Publick Advertisemt. may be Printed and Published either in the London Gazat or otherwise giving Notice that such a place is Vacant, in Dublin, That all persons qualified may be admitted to Stand Candidates for the same.

"THAT such person as shall be declared and found best qualified shall be declared professor and invested in with the same by the forementioned Guardians. If Qualified I desire the preference may be given to those descended Lineally from my Cousin-Germain Patrick Dun attested by Certificate under the hands of the Provost Bailly and Council of Aberdeen in Scotland and the Ministers of Aberdeen for the time being Next the preference to be given to those descended Lawfully from my first, second and third Sisters, attested in the like manner. Next to those descended of John Jepson Dean Willm. Jepson, my wives Sister by Dean Reader and of the Rt. Honble. the Lord Blessington and after to the best qualified without any Exception.

"THAT for salary they may receive the Rents due to me out of the Fee-farms of Temple-sonak and Currogh-bolintea The Leases to be renewed at the best improved Rent without taking any fine Gratuity or Present for the Same and if any such thing can be proved the Lease to be Voide.

"THEY yield at Present £58 Ster.

"TEMPLESONAK is Let by the present Tenant at 72lb. Yearly profit over and above all Rent to the D. of Ormond or to me. If the Leases were out they may be both Set at 200 lb. ^ʒ Ann. Clear Profit to those holding under me. Please if this may be a Competent Maintainance for two Professors or only for one.

"Sign'd at Dublin this Eight Day of June 1704 by me

"PAT. DUN. Seal.

"Pat: Mitchell

"Jon. Broadstead

"Danll. Gillispick."

This was enclosed in a paper sealed up and directed (in Sir Pat. Dun's handwriting) to His Grace the Archbishop of Dublin, and Mary Lady Dun.

So far as I am aware these important documents have never yet been printed in full; though it appears on the College Journal, under date 23 April, 1741, that 500 copies of both were then ordered to be printed; and the late Professor Osborne,* in his Annals of Sir P. Dun's Hospital, published a great part, but not the whole of this will.

* Jonathan Osborne, M.B., Dubl., 1818; M.D. 1837; sometime Pres., V.P., Censor, and Registrar of the College of Physicians. He vacated his Fellowship on election to the King's Professorships of Materia Medica and Pharmacy, and was chosen an Hon. Fell. in 1840. He was Physician to Mercer's Hospital, and published many medical essays, some of which are very curious and learned. He died 22nd January, 1864, and was buried in a vault under St. Michan's Church, where (by his own desire) his coffin stands in an upright position. He was an elegant scholar, particularly in the Greek language. For his life and writings see Dub. Quart. Journ. of Med. Sci., Feb., 1864.

Dun's friend, the Reverend Mr. King, was now Archbishop of Dublin; and the last letter of his which I have been able to obtain is written to that prelate as follows:—

“Tewsday 30 Sepr. 1712.

“Maye it please your Grace

“I am sorry to heare that your Grace hath brought ye gout to toun with you, I praye God that it maye be ane easy and short fitt, I humbly thank your Grace for your paper which is here inclosed I got Mr. Cook to draw a Coppy in wide lines I delivred it to Sr. Richard Levings, Yesterdaye he sent me notis by a Messenger that he would send me my papers in a day or two, My wife and others wer with me at the time, therefore I have writ to Lady Leving, if any papers be sent from Sr. Richard to me, that theye maye not be sent to my house but that all and Every paper maye be sent to your Grace I praye your Grace to open and read Sr. Richard's letter to me and to peruse the draught of the settlement, and to keep them by you, till it please God that we maye meet conveniently about them company is coming.

“I must Conclude that I am

“Your Grace's most devoted and faithful servant

“PA. DUN.

“If the Messenger that came from the Countrey come with the papers the inclosed piece of money is for ane acknowledgement of his trouble but if the be sent by any other they will need no gratuity but as your Grace pleaseth.

“To his Grace the Lord Archbishop of Dublin

“At St. Pulchers.”

HIS DEATH AND BURIAL, &c.

On the 20th of April, 1713, he attended a college meeting for the last time; he died on the 24th of May following, and on the 27th he was buried under St. Michan's Church, as directed in his will. Shortly after Sir Patrick's death Lady Dun addressed the following letters to Archbishop King:—

“June ye 6th, 1713.

“My good Ld.,—Could I conseall wt. has put mee into a miserable condition, out of regard to yu I would doe soe because I know in some measure yu. will greve for one of ye best of men that has bin yr. Long friend, & I am sure yr. Grace will know by barely Loosing such a man yt. I was above eighteen years a wife too and one hee valued & yt. valued him my present state is very deplorable, ye suddennes of my Change & all ye aggravations I have already known hapning to one in an ill state of health makes itt sink soe deep & Lye soe heavy on my spirits yt. unless God yt, orders all things very particularly shews his power in supporting

mee I shall not bee Long miserable, for though my reason cannot bear mee up in tryalls of ys. Life yet my Dependance on God Lets mee hope for a better, I know not wt. to say to yr. Grace I am in ye Depth of trouble, & thinke venting my self relief, I know yu. will pittie mee & I beg yu. will pray for mee, all ye hopes I have if I Live yt. I shall find yu. are trusted wth. my affairs wch. I know nothing off, I fancy my self a criminal yt. has forfeited all right to humanyty, and to every thing yt. was my own wn. I see all my house and goods Lockd up as tho. I had forfeited ym. or would steall ym., I have acted wth. Honour & while I Live will endeavour to soe in regard to his memory yt. while hee followed his own inclinations never in ye Least circumstance acted otherwise but people of principall may not suspect & see through ill designs, I have writt. a Long Letter to Lady Blessinton part of wch. I beeleave shee will acquaint yu. wth., my sad story is so cuting I would not repeat itt if I could avoid itt, I have resigned a hopefull-Child* my all in wch. I saw many misfortunes to mee and greived then for wt. I now & ever shall feell, & I have parted wth. ye best of husbands and one of ye most sincerely Just men yt. ever breathed & if instead of a Child I meet an enemy for his heir sure tis not a tryall will goe neerer to me then their Loss, I hope if I live to make a right use of itt, & see there is nothing in ys. world to make one happy since ye very actors of itt are Liable to mistakes and some of ym. soe ill as to tire ones being among them, I should bee glad to hear yr. Grace recovered yr. health wch. yt. yu. may doe & act wth. yr. former courage in all difficulties, Live Long to bee a blessing to ye world is ye hearty & constant prayer of one yt. begs yr. direction & shall doe in every action of my Life nothing hapning yt. alters my respect & value for yr. Grace but in all conditions I shall bee yr. Graces faithfull obliged

“ & very humble servt.

“ M. DUN.

“ingage some good men to befriend mee if they can on yr. acompt & his yt. is gon, I have noe merit but yt. I was ye only person hee was obliged to take care off, & is ye sufferer for his Loss.

“ ffor ye Most Revnd. his Grace ye

“ Ld. Arch-Bishop of Dublin

“ att Bath.”

“ June ye 23d 1713.

“My very good Ld.—Your absence is a very sensible addition to ye great greife I am in for ye Loss of a good husband whose virtues I soe truly valued that I thought my self very happy in a married state, & never thought enough of the condition I am come too, in wch. I have yet

* Her son, Boyle, who died young.

mett wth. noe sattisfaction but yr. Graces Letter of ye 19th wch. I this day received, by wch. I see I am not disapointed in every thing but yt. yu. are ye person nothing could alter my opinion off since I had opportunity & reason to Judge, many friends wth. yr. Graces quallifications I must not expect to meet, & but few such as yr. Grace & I have Lost I fear are Left, I am convinced yu. soe perfectly knew one anothers virtues yt. yr. friendships were sincere & if yu. are pleased to continue a demonstration of itt now hee is gone I hope I yt. am his only remains on yt. acount may have a title to yr. favour & advice wch. I intirely depend on & beg for as yr. Grace shall find I will be guided by itt I writt to yr. Grace in ye Depth of sorrow & many resentments wch. I have noe abatement off but by kind carriage of many civill friends of my own wch. I really beeleave are ingaged by ye value they had for him yt. is gone, but my Ld. I depend on yr. Grace yt. has known my behaviour as a wife to yr. friend to Judge impartially for mee, & whether yu. thinke hee valued mee or gave mee any influence over him wt. use I have made of itt is very plain to ye world, I had ever my own death and another Life too much in view to be over anxious for this if I am not provided for as well as my friends and ye world expected, I desire itt might be imputed to my own carelesnes and his weakness (wch. others watched) & my Love to him made mee as blind too as he could bee himself, upon disapointments I have taken ye way to Lessen ym. yt. yr. Grace advises & shall bee more fixt in itt since I find yu. aprove of itt, when I have found my expectations have run higher then God thought fit to answer I have endeavoured to turn my thoughts immediately on ye much greater part of ye world yt. not only want my blessings but ye necessaries of Life & by considering ym. have found great reason to bee thankfull to him yt. only knows why and how wee come to differ, I own I ought not to doubt my beeing able to act wt. part he apoints mee, and I hope I shall endeavour itt, but when a change is made yt. I am afflicted by sure I may greive without repineing for tho I feell ye stroke yet I hope tis intended for my good and will not prove ye wound of an enemy, an ill state of health makes me sink under tryals & att this time I have yt. to struggle wth., ye old attack of pain in my face are very violent, The Will yr. Grace had has bin opend I was not soe inquisitive or hasty to know itt as ye follish behaviour of some made ye town to bee, What I have to bee consern'd att in Gest now tis known, is yt. hee who was master of soe much reason and Judgment whose active Life gave him noe time to Leave remains of his knowledge but wt. ye good hee has done must keep up by gratefull people, should Leave soe imperfect a work behind him on record as this Will hapens to bee, I always knew his intent was to establish something for ye incouragement of his profession wch. hee saw by abuses was runing into contempt. I beeleave hee intended his nephew should if alive have ye first benifit of itt, wch. was

modest enough, & when wth. great difficulty I gott him to convers wth. this Nephew after yt. first ill step hee made into ye world, I fear hee talkt to him on this poynt, wt. use ye Nephew thought to make of ys. favour I am not willing to Judge, all men have their failings & by many actions I can conclude too much self is Dr. Mitchell's,* had hee acted fairly candidly & like a friend as I alway did by him hee had bin in a better condition yn. wt. hee is intended by ys. will, but hee thought hee had noe better game to play then to gett mee Lesend in my husbands essteem, hee had particular obligations to mee, & saw yt. I sett up for nothing as to my self but a comfortable subsistance during Life, & yt. att al times his uncle shewd a design of disposeing his fortune his own way wch. I never oposed nor in the Least desired to alienate any part of itt to my self. Tho I had noe Child, yet al this could not oblige him to thinke I deserved to be used as his wife, my marrying his uncle was a crime he could never forgive, My Ld. my particular greivance as to this will is my beeing Joynt executor wth. this man had I bin Left out or any indifferent person named I should have bin sattisfied, but hee will not carry himself soe as to Let mee shew ye respect due to my husbands relation, till I see yu. I cannot tell all ye reasons I have against him, & to see him soe Little thankfull for wt. he had noe pretentions too, & grudge & dispute my right yt. brought a fortune credit & was noe ill wife to his uncle & was obliged to run ye risque of his fortune will bee a great disturbance to mee, but my Ld. I hope by yr. beeing named an overseer itt will bee in yr. power to redress many of my greivances, this I please my self wth. I have never shewn a covetous temper, & I hope now there is soe Little Left mee I shall not forfeit my charactar if I now Lay hold of any advantage ye Law will give mee I should not reioyce yt. yr. Grace was to Judge for mee if I design'd or had any ill thing to effect, but I hope where I am press'd & things will bear a Just friendship yr. Grace will assist mee, had I thought yr. Grace soe ignorant of ye Will I would not have bin soe, Tho I could have trusted my spouses intent yet his weakness att ye time it was perfected has bin Laid hold on, however Just now Dr. Coghill has sworn Dr. Mitchell & I to itt, & I must tell yr. Grace I am in a much worss condition than ever yu. thought to see mee, I shall not know wt. to doe for want of yu., May yr. perfect health if I Live to see yu. return make amends for wt. I now suffer by yr. absence, I thinke my not goeing wth. yu. was a providence but beeing denyed anything by my Deare husband was very unusuall, I did not thinke I stayd to perform my Last offices as a tender nurss, I hope yr. Grace will Live Long & not know ye want of a phisition, of all people I would not have yu. my fellow sufferer, I have not words to express my

* Son of Dun's eldest sister Katherine (see note under the will).

my condition or ye respect I have for yr. Grace nothing can make mee otherwise then most faithfully my Ld.

“yr. very much oblidge & humble servt.

“M. DUN

“pardon my faults I am very full & can hardly Leave out & put together my thoughts within compass of a letter.

“ffor ye Most Revnd. his Grace ye Ld. Arch-Bp. of Dublin

“att ye Bath.”

“June ye 27th 1718.

“My very good Ld.,—I writt soe Long a Letter to yu. this last week in answer to soe good a one from yr. Grace on ye. ocasion of my sad circumstances yt. I fear yu. will wish yu. had not known mee when yu. see how much I trespass on yr. patience but my good Ld. give allowance to greif yt. is opressed by things yt. at another time would not affect or seem soe heavy, & be soe Charitable to forgive ye impertinence yt. may bee mixt wth. my great dependance on yu., my Deare husbands Will has bin read since I writ Last to yr. Grace, I find nothing could move him from an intire care & Love of mee, but by itt apears yt. hee has bin tempted to distrust mee, I have found him alter'd in his humour & way to mee for some time past but thought wth. age infirmities would come & twas my Duty to bear ym., and was so far from a Jealousie of Roguery yt. I endeavourd to have his Nephew come often to him because I found itt was agreeable to him, but my Ld. all I have suffer'd in ys. way & wt. I fear more his uncle's end was hastend by ye disquiet hee put his mind into, for after wholly trusting a wife for fourteen years wth. all hee had to be possessed wth. thoughts of her beeing false to her trust would affect any mind, I found him very often in unusuall heats, & doubts of mee, & now ye Roguery is fixt for Dr. Mitchell & his wife reports about yt. I intended Cheating him of five hundred pound yt. Mr. Dean Laid out for me in his name, Mr. Dean was an ill toll to bee made a knave off but from end to end ye story is false, wch. I would aver were I this minute to appear before ye knower of thoughts, I never had a thought yt. tended towards wronging him wch. I beelive yr. Grace or any yt. intimately knew his opinion & value for mee will beeleave because I might have influenced him to have done more for me than I could cheat him off, I hear Dr. Mitchell intends or hopes he may bring a bill against mee to force me to discover wt. money I have Laid out yt. I can sooner answer then I fear my demands will bee allowed, by ye Will hee gives mee all ye summs he had presented mee wth. had I had any design but to increase his fortune as fast as I could I might by those presents have bin very rich, but since those summs are all I am to Claim & hee thought hee had Left mee a great deall I hope wt. I will take on my Conscience to Demand will not bee denyed me, tis noe small trouble to mee to see yt. all hee has

Labourd for is goeing quite contrary to all his intents, but since tis soe I hope mine yt. is a Just Claim will not be denyed mee by trustees yt. is all ye comfort Left mee, yr. Grace Mr. Whitched and Mr. Dean or any two of yu. is to act, as yet my house is Lockt up, hee has orderd yt. all his ready money & arrears should be put out & I to have ye benefit of all above my mariage settlemt. dureing my Widowhood I beeleive yr. Grace did not thinke ys. Limitation would have bin thought of by him, yt. I should have all his plate but give security to return ye same weight to his other executors in case of marriage, his house on ye same terms, if I dye unmarried ye plate my own, I suppose he will insist on some security besides my own for ye plate, ye furniture my own wch. was all old before I had itt, ye horses & Coaches my own, I confess twas my fault I am not better in yt. particular for hee would have parted wth. these yt. are worn out & bought new but I desired to save all possible expence for this year, hee has orderd his Debts to bee paid & says noe more, I have noe money, & whether ye trustees will allow mee any till itt become next due I cannot tell, the term imployes Mr. Whitchet & Mr. Dean soe yt. they have not yet apointed a time to settle my affairs & to have ye money Lodged, if yr. Grace pleases to write to ym. wt. yu. know ye Dr. design'd not to Lay mee of all consernd in his Will under hardships & especially by those he never intended any benefitt too I beeleive they will be more inclined to favour mee where Justice will allow ym., & ye other gentleman will be ashamed of not beeing contented wth. wt. he has gott by pure ill art. I am soe very ill yt. I must gett out of town, and I am not yet resolved whether I will goe to ye Country Just for an amusement or go to ye Bath where I may hope for a more effectuall cure for ye disorders I find in my face head and indeed all over mee I beeleive all ye Juces of my body are sowerd & indeed I have hardly a thought yt. is not to that degree soe yt. I am afraid I shall not bee as much Mr. of my self as I would when I consider ye relation hee bears to one I truly loved & was happy in but when yt. ill instrument made mee other, I sadly want yr. Graces assistance on many acounts, but pray my Ld. write ye other overseers to make mee as easie as my case requires and will bear, & if I am not well & can get wherewithall to carry me I know not but ye Bath may bee ye place I shall try for my health ye next season, pray my Ld. forgive me for these sort of troubles tis ye greatest act of Charity yu. can doe to Let me communicate my thoughts to yu. I doe itt wth. yt. assurance of your Graces friendship yt. itt is a reall releife to mee in a condition yt. wants some comfort, may I have yt. of knowing yr. Grace Injoys Life & health as Long as I Live wch. is a Constant hearty prayer

“ of yr. Graces poor afflicted & sincere humble servt.

“ M. DUN.

“ Pray my Ld. if yu. see fitt to write to ym. to take care how ye money is Lodged when taken out of my house for a great deall of itt is part of

yt. mentioned to make good my mariage settlement yt. has bin paid in since this Will was made & all of itt is to bee aplied for my use, while I am a widow, I fear there will be noe care taken to put itt out by ye other executor & soe my income will bee small.

“To ye Most Revnd. his Grace ye

“Ld. Arch-Bp. of Dublin att

“the Bath.”

“July ye 2d, 1713.

“My very good Ld,—I beg yu. would accept of my hearty thanks for ye tenderness yu. express for mee in a sad condition and on an ocasion yt. I am sure if any yu. will allow I may greive, I would not have yr. Grace frighted att seeing my Leters for I really am troubled yt. I should appear anxious for ye things of a world I am not fond of Liveing in, but ye wise disposer of all things sees fitt to continue mee for reasons unknown to mee, I will endeavour all I can to bee ready for his Call, & in ye mean time since hee has put mee to act a part yt. wholly is comforted by ye promises hee has given of assisting & protecting ye widow, wth. my dependance on him I am glad I know soe good an instrument apointed as yr. Grace to apply too, I will say noe more of the value I have for yr. Grace to Lengthen this yn. to assure yu. my dependance on yr. friendship is as steady as I beeleave yr. desire of doeing good is & yt. I would not thinke for any advantage yt. yu. could bee other yu. wt. my Deare husband & I agreed in beeleving yu. were in yr. self, & to us, I beelieve twas a providence to mee yt. I mett wth. ye shocks & Loads I did wn. my affliction was soe new, itt might have quite sunk my spirits had I not bin roused & made to know yt. my condition was different from wt. it was wn. my tryall was Less but my care of my self not soe necessary as now yt. I have noe one immediately to take care of mee.

“I have read my Deare husbands Will over since my thoughts were more calm, I mist att first two things hee ever voluntary promised, one yt. no body should disturb mee for a groat att his deathe & ye other yt. in case hee had noe Child hee would Leave mee more to dispose off then I brought him, these promises satisfied mee Tho. I thought if I should ask for more there was presedents off husbands doeing more where there was noe children & wives they valued, my Ld. ye promises hee made hee intend'd to make good, & where they faill tis by my default, for hee has devised mee as a Legacy all ye presents hee made mee & bid mee keep acount off, for many years I thought ym. too great for his fortune & did not while I had hopes of Children, keep ym. distinct or Book ym. for some years past I have Tho. I Let ym. Lye in his hands or bee placed wth. his money at his Bankers, I thought ym. att first hearing ye Will all sunk, but seeing how hee had declared his trust by his Legacy to mee I copped out his Words & made ye queries I heer send yr. Grace for ye

satisfaction of my overseers who I knew would & I desired should act soe as to please all, I hope they will all thinke ye words plain enough to imply something, ye gentlemen apointed wth. yr. Grace I take to bee Judges, soe I sent to Mr. Brodrick & to Mr. Bernard who has made ye same answer & in ye same words, I cannot bee half so rich as my husband intended, for I will not Challenge any thing but wt. my word will bee Justified by my Book, Tho. I am sure itt would please him to see Justice done to mee to whome hee would still have bin more kind had hee known how things have bin carryed, but whoever is gainer I will keep to ye principalls hee trusted & wch. made us both happy in one another, & wch. I find hee still beelieved in mee tho. for some time past hee has bin often made uneasie by attacks yt. has bin made to Lessen mee to him I found when he taxed me he was soon diverted & I was afraid to know ye authour of wt. I fear'd I could not forgive, but since his Death Dr. Mitchell & his wife has fixt itt on ymselves by saying to severall they would file a bill against mee to make mee discover ye money I Laid out in other peoples names wth. a design to Cheat him yt. never denyed to give mee any thing I ask'd, I pray God forgive ym. & keep me in Charity.

"if I Live to see yr. Grace return in health I am sure I shall be revived in a great measure yr. beeing heer would have bin of great use to mee & cleared many of ye Clouds I am under, I must again return my thanks for ye care yu. have taken of mee & ye great compassion yu. exspress for my affliction, may yr. Grace Live Long to bee a support to those yt. want itt & alway be a friend to ye poor remains of yr. old friend yt. is wth. all dutyfull respect

"Yr. Graces Most oblidged & faithfull humble servt.

"M. DUN.

"pray my Ld. if yu. would have mee consult any more Lawyers & thinke these things require itt let me know, I have severall demands, one constant present hee made mee was ye fees hees had from my own relations yt. were not single & they were all I have had from him of wt. fees, he gott, unless some odd times, I am sure I cannot demand ym. wth. ye pleasure hee gave ym. mee.

'To ye Most Revd. his Grace ye Ld. Arch-Bp.

"of Dublin att

"ye Bath.

"if yr. Grace pleases return mee ye inclosed."

DISPOSAL OF DUN'S PROPERTY.

It appears from the College journal that on the 26th of October, 1713, Lady Dun addressed a letter to the President and Fellows, inviting them to use the house of Sir P. Dun during her life, and, also, to take possession of his library. On the 18th of January, 1714, the College recited Sir P. Dun's bequest of his house to them, and, in consequence of the

invitation from his widow, took possession, and met there for the first time on the same day. In 1715 a charter was obtained, dated 14th October, in the second year of George II., incorporating the professorship agreeably to the rules laid down in Sir P. Dun's deed, and appointing perpetual successorship, under the title of "The King's Professorship of Physic in the City of Dublin." The first notice for electing this King's Professor appeared on 16th of March, 1716 (*Dublin Gazette*, 1717), and Dr. Robert Griffith was chosen to be the first Professor. The electors were, Benjamin Pratt, Provost, T.C.D.; Thomas Molynaux, Regius Professor of Physic; Richard Helsham, President of the College of Physicians; William Smyth and James Grattan, Senior Censors. Dr. Griffith died in 1719, and was succeeded by Dr. James Grattan. For some years after the death of Sir P. Dun, Lady Dun was on the best terms with the College; but before the death of Dr. Griffith they found it necessary to commence proceedings by filing a bill against her and her co-executor, Dr. Patrick Mitchell, with whom (as appears by the preceding letters) she was not on good terms, in order to compel them to execute the trust. Lady Dun's own feelings will be best described in the following letter to Archbishop King; the letter itself is a good comment on Dun's intentions regarding his medical bequests:—

"May ye 3d, 1716.

"May it please yr. Grace,—I am sorry to find yr. good intention of my husband (which wholly depends on yr. Grace's friendship to him and inclination to do good) in danger of being lost. I am sensible that all which has been done since his death is owing to yr. Grace, and that the settling a Professor was a trust Left in the body of Physitians to get done previous to the Sallary he intended and I think my Life gives them an opportunity to get a Professor chosen that might have a right to Claim after I am gone and be possessed of what will fall into hands Less in their Interest than I am, but since tis thought my husbands kind disposition to me is an obstruction to this good work, Tho. as there is no present Sallary: so there is no present business required from such a Professor, but that of a Law Sute which they apprehend will be some charge, I will out of regard to my husband and gratitude to his leaving me all, give some help towards the Sute, when the Professor is Chosen and prefers a Bill to make the trustees assign according to the directions in the Will. I will Let such Professor have as much of my House for his own dwelling as will save him the Rent of a small one and in case my House should be set at the time yt. Sute commences I will allow him at the rate of thirty pound a year till the House comes into my hands and I give him an apartmt. in it, at which time ye Colledge shall have the priviledge of meeting there, and in case such a professor should have a Large Family I will quitt the whole House he paying me

twenty pound a year, and giving security for the Library of Books, this regard to my husbands intent I am incouraged to shew by yr. Graces assisting in it. I am sure all that receiv's benifit by the work must allow it a place among the many good ones that the publick owes to yr. Grace. this proposal of mine I trust your Grace with and I hope if it is Claimed on the account I have mentioned yr. Grace will not fear my going back from it, I am sure twould not be agreeable in a Letter to yr. Grace to reed the reasons I can give for my trusting all good works to yr. care I hope some that can express such things better will do you Justice and that yr. Grace will accept of my gratefull acknowledgements for yr. favours and believe me to be with all sincerity my Ld.

“Your Graces obliged and very humble servant,
“M. DUN.

“ye Ld. Arch-Bp. of Dublin.”

The case was brought to a hearing in 1723, and a decree was obtained, in pursuance of which the estates bequeathed for the support of a professor were conveyed to Lady Dun during her widowhood, with remainder to the College in trust for the professor and his successors. Another decree of the Court of Chancery was obtained in 1733, on a rehearing of the case; and this directed that an account of the personal estate of Sir P. Dun should be taken by one of the masters. When this report was brought up Lady Dun took further exceptions, and instituted proceedings afresh. At length, in 1740, by consent of all parties, it was decreed that she should assign all securities mentioned in the master's report, and also give security by recognizance for the payment of £1,200 after her death to the College for the purposes of the trust.^a

The bequest of Dun being now secured, and the college being of opinion that on the expiration of leases at Lady Dun's death the estate would rise in value, determined to enlarge the plan laid down in the deed by establishing professorships supplementary to the medical courses then taught in the university; and in 1743 obtained an act of Parliament^b vacating the office of King's Professor on the death or resignation of Dr. Grattan; and directing the annual income previously applied to that purpose to be divided equally between three professors, who were to be

^a Osborne's *Annals of Sir P. Dun's Hospital*, p. 11. The College journals state that on the 18th of January, 1741, the president was served with an ejectment from the ground on which Dun's house stood.

^b This act was never printed among the statutes of Parliament; and though frequently quoted as having passed in the 21st of Geo. II. (even in subsequent statutes, such as the 40 Geo. III., cap. 84,) it was enacted in the 15th of Geo. II., and is expressly stated to be a *public act*. I have read the original in the Record Office. It appears to have been privately printed, in 8vo, in 1747. See Dr. Perceval's "*Account of the Bequest of Sir P. Dun*," &c. Dublin, 1804. There is no printed copy extant, so far as I know.

styled the King's Professors of Physic; of Surgery and Midwifery; and of Pharmacy and Materia Medica. The candidates were to be examined by the Provost of T.C.D.; the Regius Professor of Physic; the President of the College of Physicians, and the two Senior Censors of the same. The college journals state that this bill passed on 18th January, 1741, In January, 1748, Lady Dun died, and was buried under St. Michan's Church, beside her husband.*

Dr. Grattan, the second King's Professor, died in the same year; and on the 20th of May, 1749, the first notice for the election of Professors under the Act of Parliament appeared.^b

The following were elected:—

Professor of Physic—Henry Quin, M.D., of Padua.

„ Chirurgery and Midwifery—Nathaniel Barry, M.D., of Rheims.

„ Pharmacy and Materia Medica—Constantine Barbor M.D., of Dublin.

On the 28th of November, 1752, each of the King's Professors was ordered to lecture thrice weekly in the Philosophy School, T.C.D.

Dr. Barbor died in 1783, and the estates at this time producing £926 per annum, the College of Physicians conceived that the School of Physic ought to be furnished with additional professorships, and provision should, be made for clinical instruction. The following notes, which I have taken from the Parliamentary papers at Dublin Castle, show the preliminary steps which eventuated in the passing of the Statute 25 Geo. III., cap. 42, in 1785:—

1783-4.—Committee of House of Commons appointed to enquire what may be the most effectual means of establishing a complete School of Physic in this kingdom (No. 1, Bundle 94, Com. Books). Monday, Dec. 1783—Rt. Hon. J. H. Hutchinson in the chair.

Dr. Cleghorn examined—Objected to then existing plan of School of Physic as incomplete—“Degrees not being founded by it.” He also thought it not reputable that gentlemen should go abroad for degrees. Dr. Cullen said “that there is nothing mentioned in the will of Sir

* “In Summer, 1843, the writer of this note, accomponied by a gentleman connected officially with Dun's Hospital, having been furnished with the requisite permission from the parochial authorities of St. Michan's, made an unsuccessful search for the bodies of Sir Patrick and Lady Dun, in the vault stated to have been purchased by the deceased knight for his own interment and that of his wife, and especially ordered by his will to have been closed; a direction which was found to have been entirely neglected; so ignorant, indeed, on this topic, were the persons in charge of the church, &c., that it was only on referring back to the church books in the years in which Sir Patrick and Lady Dun had respectively died, that it was made apparent to the parish officers of St. Michan's that the couple in question had been interred below the church.”—“A Short Memoir of James Young, &c.” (*op. supr. cit.*) note G., p. xii.

^b Book of Electors Proceedings.

Pat. Dunn about the study of medicine." Testator everywhere calls it a professorship which he wished to establish in the College of Physicians. In Edinburgh the proportion of medical students desiring degrees to those who did not [*i.e.* those wishing licences only] was "about 24 to 300."

This committee adjourned to 27th June next; but no report appears to have been given. After some delay the Act already referred to was passed. This statute changed the University lectureships of anatomy and chirurgery, chemistry, and botany, into professorships, and instead of the professorships previously established it constituted the following:—Institutes of Medicine, Practice of Medicine, Materia Medica and Pharmacy, Natural History, and, contingently, Midwifery. It also made provision for clinical lectures, and for support of Sir P. Dun's library. This act was (*inter alia*) intended to remedy abuses which existed with reference to the discharge of the professorial duties. In the records of a Committee of the House of Lords, which sat on 22nd July, 1756, it appeared from the evidence of Dr. Anthony Relhan, president, and Dr. Barbor, vice-president, of the College, that no lectures had been read as provided by statute. The first professors under the 25 of Geo. III., cap. 42, were appointed in March, 1786. They were:—

Institutes of Medicine—Stephen Dickson.

Practice of Medecine—Edward Brereton.

Materia Medica and Pharmacy—Edmund Cullen.

The other professorships do not appear to have been filled up.

In 1791 a Committee of the House of Commons was appointed to consider the best mode of promoting the execution of the Act for Establishing a School of Physic, so far as it relates to Clinical Lectures (B. 94, von). It met 26th March, 1791, Right Honble. Secretary of State in the chair. Drs. Brereton, Percevall, Stephen Dickson, and Edward Cullen, examined.

The committee reported that an hospital ought to be built, and that the College of Physicians should be empowered to do it; and they recommended a bill to this effect to be brought in. This was done; and a further statute was passed in the 35th of Geo. III. empowering the College to raise £1,000 on Dun's estates to build an hospital. The construction of the 25 Geo. III., on which this was based, was contested at law by the professors; and so nothing came of this last statute.

On the 15th of April, 1799, a committee of the Irish House of Lords was appointed to enquire into the application of Sir P. Dun's funds, and to report how far it is consistent with the public good and with the faithful discharge of the intentions of the testator that the appropriation of the said funds should remain any longer in the College of Physicians. This committee met; and as there is even now considerable diversity of

opinion on this question, I subjoin extracts from the evidence on which its report was based :—

Dr. Perceval was examined, and deposed that he did not think the College had faithfully discharged the trust or the intentions of Dun. He stated that the surplus had been applied “partly in purchasing books, partly in the payment of the library porter, and attendance upon the library.” The largest number of patients admitted to the temporary hospital^a was 33; the smallest, 1; the average, 18. He did not think the College should continue to dispose of Dun’s funds, except under the control of visitors, and by the exclusion of interested persons from their body. Dr. Harvey stated that there were then no regular courses of lectures given. In reply to the query, “Could any useful or extensive course of lectures be applied to the case of one patient?” the following replies were given :—

A. “Dr. Plunket.—I think not.

“Dr. Cullen.—Certainly not.

“Dr. Harvey.—Yes; if a very curious case?”

It was further deposed that the annual support of each hospital bed in Dublin in 1799 was £15; and that the annual salaries of the King’s Professors were then £300. Drs. Hopkins, Cullen, and Harvey agreed that the application of Dun’s funds to a medical library was in accordance with the intention of the legislature, with Dun’s will, and for the promotion of the study of physic. The following query and replies bear on the present state of the School of Physic :—

Q. “If the two studies of surgery and physick were united, and an establishment made for both under one roof, don’t you conceive it would tend most materially to extend the knowledge of physic, and be highly beneficial to the nation under wise and proper regulations?”

A. “Dr. Hopkins—I do think it would.

“Dr. Cullen.—I do, and accordingly it has been begun by grafting it on Mercer’s Hospital.

“Dr. Harvey.—I do not think myself competent to answer this question.”

The following is the report of the Committee :—^b

April 18th, 1799, Committee met.

Earl of Altamont in the Chair.

Present :—Earl of Mayo, Lord Tullamore.

The following report of the examination and inquiry into the application of the funds bequeathed by Sir Patrick Dun was agreed to :—

My Lords,—The Lords Committees appointed to examine into the

^a Of this more hereafter.

^b Committee book, No. 9. Index to Irish Parl. Records. Com. books and papers. Charities Bundle 8, press M., shelf 4.

application of the funds bequeathed by Sir Patrick Dun for the establishment of an hospital for clinical lectures,^a and to report the same, as they shall appear to them, to this house, have met and made a minute inquiry into the matter to them referred, and after an investigation of the books of the College of Physicians, and the examination of the most respectable members of said college,^b as well as of the Professors of Physick by them chosen, whose testimony is now submitted to your Lordships, it appears clearly that the intentions of Sir Patrick Dun, as explained by the Acts of the 25th and 31st of the present reign, have not been carried into effect, and, by the unanimous admission of every witness examined, the trust confided in the said College of Physicians to have been grossly abused.

It appears to your Committee that by the 31st of the present King it is provided that salaries from the funds of Sir Patrick Dun shall be paid to the three professors at the rate of one hundred pounds each, and no more, and that the surplus of the income of said estate, which exceeded one thousand pounds a-year after paying the said three professors, should be applied to the establishment and support of an hospital as the best means of extending the knowledge of medicine by uniting the practice to the theory of physic.

It appears to your Committee that the salaries to the said three professors, at one hundred pounds a-year, and no more, has been regularly paid, but that though no hospital has been permanently established, nor any more than a very small sum applied to the support of patients, the only balance of the said surplus now forthcoming is £5 9s. 3d., though there ought to have been a balance of many thousand pounds.

In searching for the cause of said deficiency, it appears to your Committee, that many considerable sums have been expended by said College of Physicians, not at all warranted either by the intention of the testator or by the several acts of the Legislature before alluded to for carrying the same into effect; and among the said items unwisely and unwarrantably expended, your Committee hold themselves bound to notice a present of claret to the President of the College of Physick annually; an immoderate purchase of books, in some instances twice paid for; lawsuits carried on in which the said College were both plaintiffs and defendants, and actually paid from said funds the expenses of both; and loans to indigent members of said College, which were never repaid in many instances, and which, with other charges equally foreign to the said trust, have consumed the whole surplus income of Sir Patrick Dun's estates, which under wise and frugal management would have afforded means for a great and useful national establishment.

Your Committee being of opinion that there were funds abundantly

^a Compare the terms of his deed.

^b Dr. Hill in his pamphlet quoted below, arraigns the evidence as one-sided.

sufficient for such establishment, earnestly hope that the wisdom of the Legislature will put them under such Regulations as will faithfully discharge the benign intention of the testator, and most extensively benefit the public.

With reference to this report, which contains a flat contradiction to Dun's own words, as regards his intentions; I may say that, on perusal of the College journals, it is evident that there were two parties at this time in that body. Dr. Perceval, and those who sympathized with his views, were in the minority; and he seems to have resorted to legal means, on the failure of frequent attempts in the College, to carry his own views. At any rate, the College censured him for his share in this transaction; but yet he very soon after was elected President by them. His influence was very great, chiefly because he was supported by the authorities of the University, who thus created a bad precedent for their own interests; but whatever may be said for the success attending his efforts, one thing is quite clear, that the statute 40 Geo. III., cap. 84 (now known as the School of Physic Act), which was enacted in 1800, and was, I believe, almost the last act of the Irish Parliament (of which it was quite worthy), applied Sir P. Dun's funds in ways never intended by him—virtually transferred them from the body designed by him to two others which were never intended to profit by them, and introduced an unsound principle, by thus proving that no individual can safely leave his property for any public purpose.* Section 8 of this Act furnishes sufficient evidence on this point, by coolly transferring the property of one body to another:—

“And whereas a sum of one thousand two hundred pounds stands vested in the public funds of Great Britain, which belongs to the said College, and constitutes a part of the funds applicable by them to the purposes of their Institution; be it enacted that the said Commissioners [certain persons named in the Act to manage the building of Sir Patrick

* From his will and deed it is manifest that he left his money to advance the position of his own profession by providing professorships of value which would raise their holders, and the calling of medicine, in the estimation of the public, and also raise the standard of medical education. So, indeed, Lady Dun says in one of her letters. He never contemplated the support of the sick poor; and the legal injustice done by forfeiting his funds (in great part) for the last-named purpose is much the same as if the greater part of the fund left by his friend Archbishop King to endow a Divinity Lecturer were given by Act of Parliament to build and endow churches in Dublin, because the fund was very valuable, and the lectures were irregularly given. On this subject see on one side, Dr. Dickson's "Letter to his Medical Brethren relative to the School of Physic." Dub., 1795; and Dr. Hill's "Address," and other pamphlets. Dub., 1803, 1805, and 1814; on the other side, Dr. Perceval's Account of Dun's Bequest. Dublin, 1864. All are in the Library of Coll. of Phys.

Dun's Hospital] shall be, and they are hereby empowered to cause the said stock to be sold for the best price which can be had for the same, and to apply the produce to the like purpose of building and completing such Hospital."

According to Dr. Osborne's *Annals of Sir Patrick Dun's Hospital*, this was actually done.

The School of Physic Act of 1800 is the statute under which Dun's property is now administered; and it is generally called by the above title, because it has reference to other matters than Dun's estates, and chiefly to the constitution and regulation of that medical corporation or college which is therein entitled "the School of Physic," but which is commonly and incorrectly confounded with the Medical School of the University. The bill was read for the first time in the House of Commons, on 2nd July, 1800; for the second time, on the 4th, on which day it was committed. It passed through committee, and was reported on the 8th; was read for the third time and passed on the 9th; was returned to the Lords and assent given out Aug., 1800.*

In order to give a clear view of a somewhat complex question I may briefly recapitulate the corporate and legislative transactions in regard to Sir P. Dun's Will.

By his will he directed that his intentions as expressed in his "deed" already given, should be carried into effect. This was partly done by authority of a Royal Charter which was, in effect, nothing more than giving an official stamp to Dun's will.

This was succeeded by the Act passed in the 15th year of Geo. II., which also adhered to Dun's will, except in two important particulars:—
1. It provided that his intentions respecting the teaching of anatomy, botany, &c., need not be carried out, inasmuch as the university had made provision for this part of a medical education; and 2. It increased the number of professors beyond those contemplated by Dun, to meet what was certainly a pressing want at the time. On this latter fact I may observe, that the Legislature and the College of Physicians by appointing a Professor of Chirurgery and Midwifery, carried out the old idea, which is becoming fashionable again in our own day, that chirurgery and midwifery were parts of physic, not separate professions; and, it will be borne in mind that these professors were appointed to teach students in *physic, surgery, and pharmacy*.

The Act of the 25th of Geo. III., altered and amended the Act before mentioned, chiefly by abolishing the Professorship of Chirurgery and Midwifery, and by instituting instead of it one of the Institutes of Medicine, and one of Natural History. The Act of the 31st of Geo. III.,

* Vol. xix. of Journals.

amended the last mentioned statute chiefly in respect of that part which related to clinical lectures; but the Act of 40th Geo. III., cap. 84, repealed all the preceding statutes on the same subject, *except* a portion of the statute of the 15th Geo. II.; and, as this Act was never printed, among the statutes, I subjoin those portions of it which are yet unrepealed.

Among the enactments regarding the examinations for selections of professors is this clause :—

“And be it further enacted by the authority aforesaid that if, upon any election of the professor or professors aforesaid, there be any person among the candidates who shall be lineally descended from the aforesaid Patrick Dun, of Taerty,^a and shall be attested to be so by certificate under the hand of the Provost Baillies and Council of Aberdeen in North Britain, and the Ministers of Aberdeen for the time being, and who, on such examination as aforesaid, shall be found to be equally qualified with any of the other candidates for the said professorship or professorships, and shall be so reported by the examiners aforesaid, such person or persons shall have the preference before any other of the candidates, and shall be by them declared duly elected into the said professorship or professorships; and such person or persons, so reported and declared, are hereby respectively declared to be the professor or professors within this Act; anything herein contained to the contrary thereof, in anywise notwithstanding, and the [that?] next to those descended from the said Patrick Dunn the like preference shall be given in like manner to any other candidates who shall be examined and reported equally qualified as aforesaid, who shall be lineally descended from the first, second, and third sisters of the said Sir Patrick Dunn,^b the issue of the eldest and their issue being always to take place of the issue of the youngest and their issue; such descent from the said sisters of the said Sir Patrick Dunn to be certified and attested as the descent of the said Patrick Dunn of Taerty is hereby directed; and the like preference in like manner shall be next given to such candidate as shall be lineally descended from the said John Jepson; and after them to such as shall be lineally descended from the said William Jepson; and next to them, to such as shall be likewise lineally descended from the said Elissia Reader, sister of the said Dame Mary Dunn, and wife of the said Enoch Reader, deceased; and lastly, to those descended from the said Murrough Lord Viscount Blessington, in the order and precedence and according to the priority of their respective nominations as they are last before mentioned herein and to no other.”

In a book which I have previously quoted^c it is stated “that the

^a Sir Patrick Dun's cousin mentioned in his Will.

^b See notes to the will.

^c Memoir of James Young, &c.—Note G.

above-mentioned right of preference enjoyed by the parties referred to in the deed of Sir Patrick Dun [and confirmed by Royal Charter, and by Statute of 15 Geo. II. just quoted] was fully recognised by the authorities of the hospital, when in the Winter of 1831–32 the late William Ross, M.D., son of John Leith Ross, of Arnage, and Mrs. Elizabeth Young, of Bourtie—the latter a great granddaughter of Mrs. Elizabeth, or Bessy, Dun—was for a period extending over several months following his medical studies in the Irish capital.”

I have searched the hospital records in vain for any confirmation of this statement; but I find that a Mr. Charles A. Ross is entered in the pupil's book as having paid his fees to the hospital on the 1st of November, 1834.

Regarding Dun's Library the following clause of the same Act is still in force :—

“And as to and concerning the library of books, part of the said personal estate of the said Sir Patrick Dun, so vested in the said College of Physicians as aforesaid. Be it enacted, by the authority aforesaid, that the said President and Fellows of the College of Physicians in Ireland shall, with all convenient speed, by and with the consent of the said Archbishop of Dublin then being, and any two of the said professors, deposit and place the same in some convenient place in or near the said City of Dublin, for the use of the said College of Physicians, and of all the said professors and their successors, subject to such conditions, rules, regulations, and directions for the safe custody and good usage of the said books, as by the said Archbishop of Dublin, the said President and Fellows of the College of Physicians in Ireland, and any two of the said Professors, by deed or instrument in writing, under the common seal of the said college, and under the hand and seals of the said archbishop and professors, shall be limited, appointed, and established concerning the same.”

THE SCHOOL OF PHYSIC.

“The School of Physic in Ireland” is a medical corporation or college, governed by the Statute of the 40th Geo. III., the contents of which are well known to all concerned. It consists of the Professors of Anatomy and Chirurgery, of Chemistry, and of Botany, on the University foundation, appointed by the Provost and Senior Fellows of Trinity College; of the King's Professors of the Institutes of Medicine, of the Practice of Medicine, and of Materia Medica and Pharmacy, on the foundation of Sir Patrick Dun, practically appointed by the President and Fellows of the College of Physicians, as provided in the School of Physic Act;^a and

^a There are five electors; the Provost of T.C.D.; the Regius Professor of Physic; and three (the majority) chosen by the College of Physicians.

of such students as shall matriculate with the Registrar of Trinity College.

To this Statutable foundation the University has added a Professor of Surgery, an Anatomist, and an Assistant Anatomist, all on the foundation of Trinity College; and the College of Physicians has added a Professorship of Midwifery and the Diseases of Women and Children; and also a Professorship of Medical Jurisprudence. As soon as the Governors of Sir P. Dun's Hospital are able to support 100 patients, the Professorship of Midwifery will become a King's Professorship on the same foundation as the others of the same name.

Full details of the working of the School of Physic, and of the succession of the University professors and lecturers, will be found in the Dublin University Calendar; the following is the list of professors on Dun's foundation, and of those founded by the College of Physicians:—

PRACTICE OF PHYSIC.

Robert Griffith,	.	.	Elected 1717.
James Gratton,	.	.	„ 1719.
Henry Quin,	.	.	„ 1749.
Edward Brereton,	.	.	„ 1786.
Stephen Dickson,	.	.	„ 1792.
Whitley Stokes,	.	.	„ 1798.
Martin Tuomy,	.	.	„ 1812.
Chas. R. A. Lendrick,	.	.	„ 1832.
George Greene,	.	.	„ 1841.
John Thomas Banks,	.	.	„ 1846.

CHIRURGERY AND MIDWIFERY.

Nathaniel Barry,	.	.	Elected 1749.
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MATERIA MEDICA AND PHARMACY.

Constantine Barbor,	.	.	Elected 1749.
Edmund Cullen,	.	.	„ 1786.
John Crampton,	.	.	„ 1804.
Jonathan Osborne,	.	.	„ 1840.
Aquilla Smith,	.	.	„ 1864.

INSTITUTES OF MEDICINE.

Stephen Dickson,	.	.	Elected 1786.
John Wm. Boyton,	.	.	„ 1812.
William Stack,	.	.	„ 1826.
Robert James Graves,	.	.	„ 1827.
Robert Law,	.	.	„ 1841.

NATURAL HISTORY.—None appointed.

MIDWIFERY (COLLEGE FOUNDATION).

William Fetherstone Montgomery,	.	Elected	1827.
Fleetwood Churchill,	.	„	1856.
Edward Burrowes Sinclair,	.	„	1864.

MEDICAL JURISPRUDENCE.—(College Foundation).

Thomas Brady,	.	Elected	1839.
Robert Travers,	.	„	1864.

NOSOCOMIUM PATRICII DUN. EQ. AUR.

(*Sir Patrick Dun's Hospital.*)

Sir Patrick Dun's Hospital was founded by the School of Physic Act in 1800, and is now governed by the directions laid down in that statute.

For several years previous to its erection, great inconvenience was felt from the want of clinical lectures in connexion with the School of Physic; and several temporary expedients were resorted to to remedy this defect. The statute 25 Geo. III., provided that until a hospital should be built, clinical lectures might be given in some hospital in Dublin. Accordingly, on the 14th of January, 1788, the College of Physicians to some extent fulfilled the spirit of the law by taking a house in Clarendon-street for a clinical hospital. After a time this had to be given up, chiefly on the score of expense, a committee reporting to the College, on the 14th of August, 1790, that the cost of maintaining the institution was much higher than in other hospitals in Dublin—the cost of drugs alone being *eight times as high* as at other hospitals. On the 8th of November, 1790, the furniture of the hospital in Clarendon-street was ordered to be removed to Mercer's Hospital: and in case it could not be there accommodated, further directions were given to remove it to Stevens' Hospital. On the 9th of July, 1792, a lease of a house on Blind-quay was accepted by the College for an hospital. This was called Sir Patrick Dun's Hospital, and was put in working order for thirty clinical patients on the 27th of September in the same year. This institution seemed to work for a short time only, for, on the 16th of February, 1793, the College took into consideration the appointment of a Physician-in-Ordinary to Sir P. Dun's Hospital in Lower Exchange-street. According to the College journals, this was working in 1797; and on the 21st of January, 1799, Mercer's Hospital was reported ready for the reception of clinical patients on Dun's foundation. The College and the Governors of Mercer's Hospital did not pull well together.

At first the hospital in Lower Exchange-street was kept open during the whole year, then only during a part of the year; next, the patients decreased in number, until, finally, it would appear from the evidence

given before the Lords' Committee in 1799, that at one time only one patient was to be found in it.^a

By the statute 31 Geo. III., the College was empowered to raise £1,000 to build an hospital; but, as already stated, the professors contested the construction of this Act, and so nothing was done in that way until after 1800, when the School of Physic Act gave more clear and extended powers.

By this Act the building of the hospital was entrusted to eight Commissioners, viz., the Right Honourable Sackville Hamilton, the Provost of Trinity College, and the President of the College of Physicians for the time being, Sir Francis Hutchinson, Baronet, the Hon. George Knox, Dr. Arthur Browne (LL.D.), William Digges Latouche, and Abraham Wilkinson, Esquires. These Commissioners were directed by the Act to "appropriate" the £1,200 already mentioned, belonging to the College of Physicians, together with the rents which should be collected (minus payment of certain fixed charges), until accommodation should be afforded for thirty patients. They selected a lot of ground in Grand Canal-street, of which a lease for 998 years was granted to the President and Fellows of the College of Physicians, from David Courtenay, Esq. The College seal was affixed to this lease on the 10th of May, 1862. "Having in 1808 expended £6,346 of Sir Patrick Dun's funds in the erection of the west wing of the hospital, and in enclosing the ground, and making sewers, and, being destitute of any means for carrying on the building, except the annual produce of the estates, which was quite inadequate, an application was made for Parliamentary aid, and in the ensuing session they obtained a grant of £6,204 for the purpose of finishing the wing then erecting, and for procuring, furnishing, and maintaining a temporary hospital."^b—Osborne's *Annals*.

On the 24th of June, 1808, the west wing being finished, and capable of affording accommodation to thirty patients, the Commissioners summoned a Board of the Governors nominated in the School of Physic Act, and delivered up to them the management of the institution.

By the aid of subsequent Parliamentary grants the hospital was completed, and accommodation was provided for the Professors on Dun's foundation, who lectured there from that time until a few years ago—for the students, for Dun's library, and for the Convocation Hall of the College of Physicians. The building of the hospital was commenced in

^a This record of the various attempts at clinical instruction has been taken from the College Journals, and from the pamphlets of Drs. Dickson, Hill, and Perceval, already noted. The hospital of Blind Quay is not noticed by these gentlemen; and wherever any discrepancy occurred in the accounts, I have followed the minutes of the College of Physicians.

^b See Report on certain Charitable Establishments in the City of Dublin, which received Aid from Parliament. Dublin, printed by Grierson in 1809.

1803, and was completed in 1816, and the first College meeting there took place in 1818. The Board of Governors consists of the Visitors of the College of Physicians; the President, Vice-President, and Censors of the same; the Provost of Trinity College; and of twelve other persons chosen by the *ex-officio* Governors from among the subscribers to the hospital.

The elected Governors are annually chosen, in March; and though the annual election does not appear to be required by the Act, yet it has been the invariable custom. In point of fact, however, it does not make any material difference, as it is usual to re-elect the same persons from year to year, during their lives. "No Physician or Surgeon who shall attend Patients [in the hospital], shall be capable of acting as a Governor of the said hospital."—40 Geo. III., cap. 84, sec. 4.

During the last two years the College has given over to the Governors the entire use of the central portion of the building, with a view to the conversion of the hospital into a medico-chirurgical institution, a change which was considered desirable for the improvement of the School of Physic, medical cases only having been admissible previous to this alteration. This change has been attended with the best results, and has contributed, in some degree, to carry out the intention of Sir P. Dun, who—as appears from his will and deed—considered surgery to be a part of medicine, an educated view of the subject which Hippocrates and the ancients entertained, and which the strides of modern medical science have shown to be the correct view. So long ago as the 16th of February, 1793, physicians-in-ordinary were recommended to be appointed to attend patients in the temporary Dun's Hospital in Lower Exchange-street, and this usage has been maintained almost to the present time. In consequence, however, of the conversion of the entire hospital to clinical purposes under the late arrangement, the clinical professors who, previously, had only attended cases selected for clinical purposes, will, in future, have charge of the whole hospital; and the office of Physician-in-Ordinary will terminate with the term of years for which each of the present holders, Dr. Henry Kennedy and Dr. William Moore, was appointed.*

The intention of the School of Physic Act was that the University Medical Professors named therein, and those on Dun's foundation, should be also clinical teachers, in accordance with the then fashionable Scotch system, which was further complied with in the adoption of their medical "session" into our school of physic instead of the university "terms," previously in use in Irish medical education, as in that of the other

* For the list of Physicians in Ordinary, and Extraordinary, and also the lists of all the other officers of the hospital, see a MS. vol., "Tables of Useful and Interesting Memoranda concerning Sir Patrick Dun's Hospital," which is in the custody of Mr. Joseph Mullen, the Registrar.

professions. If all the professors were chosen with this view it would answer well; but it is not to be expected in the present state of science that a man chosen specially to teach chemistry or botany should, as a matter of course, be a good clinical teacher also. Nor, on the other hand, can it be expected that the best teachers of these and other important sciences accessory to medicine should be at the same time the men best fitted to give clinical instruction.

Among the numerous pupils of Sir P. Dun's Hospital, the following, from 1827 to 1849, are well known to the profession.—See Pupil's Book.

Henry L. Dwyer.

William Barker.

Robert W. Smith.

John T. Banks.

Fleetwood Churchill.

George Green.

Robert John Kane.

Hans Irvine.

Robert Bentley Todd.

Charles Lever.

Aquilla Smith.

Alfred Hudson.

M. H. Stapleton.

Denis Chas. O'Connor.

Eugene Finn.

Thomas Brady.

Cathcart Lees.

John Popham.

James F. Duncan.

Robert Travers.

William Reeves.

Horace T. N. Meade.

Wm. E. Steele.

Henry Kennedy.

Robert D. Lyons.

Edward B. Sinclair.

Maurice H. Collis.

Lombe Atthill.

Armstrong Todd.*

SIR PATRICK DUN'S LIBRARY.

"*Bibliotheca Patricii Dun, Equitis Aurati*," was founded in 1711, by the will of Sir P. Dun, who bequeathed his books to the college; and this bequest was subsequently confirmed by the Act of Geo. II., the special clause of which referring to this subject I have already given in full; as it is still in force. In the clause aforesaid the books are stated to be for the use of the president and fellows and of the professors on Dun's foundation. By the statute 25 Geo. III., cap. 42, part of the surplus funds of the trust estate were directed to be used "for purchasing medical books for the use of the students in physic" (sec vii.), and considerable sums were expended from time to time in pursuance of this enactment. The School of Physic Act, 40 Geo. III., cap. 84, wholly repealed that of 25 Geo. III., cap. 42, and did not substitute any corresponding clause; but the equitable principle therein contained, viz., the right of students of the school of physic to use Dun's library, has all along been admitted by the president and fellows, who have never ceased to admit to the

* This list, taken from the Pupil's Book, is very incomplete.

library, under certain conditions, those who have matriculated in that school according to statute.

As already stated, the School of Physic Act of 1800 placed it in its present position, and under that statute, and the unrepealed portion of the 15th of Geo. II., it is now supported and governed. The Act of 1800 does not directly provide for its support out of Dun's estate, further than by providing for a librarian and some minor matters; but from 1800 to the present time a small annual grant for purchasing books and paying a library servant has always been made by the college, and allowed by the visitors, under the head of "other matters incident to the school of physic," to which, according to sec. 4 of the Act, part of the surplus of the estate may be applied.

For some years after Sir Patrick's death the library remained in his house, which was used by the college as their place of meeting. Before a Committee of the House of Lords on the 22nd of July, 1756, the following evidence was given:—

"Q. Archbishop of Dublin.—What is become of the library of Sir P. Dun?

"A. Dr. Anthony Relhan.—The remains of them [are] in the possession of Dr. Quin, as they have no other place.

"Q. Have they any catalogue of the books?

"A. Yes; they have.

"Q. How many books are there?

"A. About 300.

"Q. If any security was ever given for the forthcoming of the books?

"A. It was never required."

By a subsequent statute the books were directed to be removed to a room in Trinity College, where they remained until 1815, when, in accordance with the provision of the act of 1800, they were again removed to an apartment provided for them in the hospital. When the new college premises were opened in Kildare-street, in 1864, it was deemed advisable to have the library placed there; and as the Act of 1800 did not direct that they should be permanently placed in the hospital, while that of 1743 directed their removal to "some convenient place in or near the City of Dublin," they were removed to their present more useful and "convenient" position in the Summer of 1865.

From the Lords' Committee report of 1799 it would appear that great abuses had existed in regard to this library; but these have long disappeared.

On the 14th of August, 1787, the first library regulations appear to have been made. They are given at length in the College journal of that period; and at the same time £18 a year was allowed for a library porter,

as well as considerable sums for purchasing lecture apparatus for the use of the King's professors.

At first one of the King's professors filled the office of librarian; but subsequently this office was filled by one of the fellows. The following is a complete list of the librarians:—

Stephen Dickson,	.	.	Elected 19 Sep., 1787.
John Wm. Boyton,	.	.	„ 23 May, 1797.
Hugh Ferguson,	.	.	„ 18 Oct., 1808.
Peter Edward M'Loughlin,	.	.	„ 18 Oct., 1815.
Francis Hopkins,	.	.	„ 19 Apr., 1819.
Edward Hill,	.	.	„ 22 Nov., 1819.
Thos. Herbert Orpen,	.	.	„ 18 Oct., 1826.
Samuel Litton,	.	.	„ 18 Oct., 1833.
John O'Brien,	.	.	„ 18 Oct., 1840.
Geo. Alex. Kennedy,	.	.	„ 5 Jan., 1846.

None but a fellow was ever chosen to this office; but since 1800 the fellow so elected had, at once, to vacate his fellowship in accordance with the terms of the School of Physic Act.

Up to the Summer of 1864 it was only a lending library, but on the death of Dr. Kennedy, which occurred in 1865, it was resolved to convert it into a reading library also. Accordingly the position of librarian had to be modified. A layman was elected to the office of librarian on Sir P. Dun's foundation, with duties similar to those usually discharged by the assistant librarians in our large public libraries. In addition the College created the office of Honorary Librarian, which was intended to be filled by one of the fellows, who need not resign his fellowship; and who should have the position and duties of senior or chief librarian in our large public libraries. The title "Honorary" was given to denote that the holder received an *honorarium*, or fee, for his services from the college private funds, as distinguished from the librarian who receives a *salary* from the trust estates.

On the 8th July, 1865, the College elected—

Hon. Librarian—T. W. Belcher.

Librarian on Dun's foundation—H. J. Fennell.

The library was opened in the new College in October, 1865, and it is open daily from 10 a.m. to 6 p.m., Sundays and a few other days excepted.

In the convocation hall of the College of Physicians may be seen a fine portrait of Sir P. Dun in the robes of a Doctor in Physic, painted by Sir Godfrey Kneller. The accompanying engraving, by Mr. W. H. Lizars,*

* This first appeared in the Dublin Quarterly Journal in 1846.

gives a very good idea of it, and also a *fac simile* of Dun's signature. Professor Osborne, in his *Annals of the Hospital*, mentions another portrait of Dun which was in possession of John Leith Ross, Esq., of Arnage, in 1844.

The picture of Sir Godfrey Kneller was presented by Peter Walsh, Esq., of Belline, and arrived in Dublin on the 14th of July, 1817. In the *College Journal*, under that date, appears a copy of the following certificate :—

"I certify that the portrait which I have the honour of presenting to the College of Physicians in Ireland is a genuine (copy) and original portrait of Sir Patrick Dun. It remained many years among the family pictures in the possession of the representative of Lady Dun's sister (who survived her), and remained so when I procured it from a person that obtained it from Mrs. Jepson, the widow of a grandnephew of Lady Dun. The picture itself bears internal evidence of its authenticity.

"Belline December 12th—1816.

"PETER WALSH."

It may be well to give a short summary of a few facts connected with the College history from Dun's time to the present. Repeated efforts, commencing in 1695, were made by the College to get the charters of William and Mary confirmed by statute. These were petitioned against, mostly by the apothecaries, and invariably defeated.^a Several statutes were, however, passed during the 18th century, which had the effect of confirming the most important parts of the charter; and for a more particular account of them I must refer the reader to my *Historical Introduction to the College Register* for the present year. On the 23rd September, 1698, the Fellows were ordered to meet on Wednesdays and Fridays, at 8 p.m., at "St. Laurence's Coffee House, during next session of Parliament."

From the temper of the times, and the previous history of the College, it was to be expected that some Fellows under the Old Charter were excluded by the new; indeed this is implied on the preamble of the latter.^b Accordingly, in the *Journal* of 15th April, 1700, it was resolved :—"That it be considered at next solemn meeting how ye fellows of ye Colledge shall behave yurselves towards such physitians as have been of ye former Charter and towards papist physitians."^c

^a See Dr. Moore (W. D.) "On the History of Pharmacy in Ireland" (*Dub. Qu. Journ.*, Aug., 1848). This is one of the most interesting of Dr. Moore's many and valuable papers.

^b Sec. 3.

^c Nothing seems to have come of this notice of motion. This probably occurred through the influence of Dun, who in the matter of Dr. Crosby, already detailed, and in other things, took a more liberal view.

In the Lords' Committee Report, which preceded the passing of the Act, 1800, it was complained, among other abuses, that part of Dun's money was spent in buying claret for the President. Regarding this, it appears that the first hogshead of claret was ordered for President Harvey on the 27th of September, 1792; and on the 15th of December, in the same year (being the centenary of their incorporation), the College dined at Keane's house in Kildare-street. For many years after 1800 the system of giving each president £100 for claret for entertaining the Fellows during his year of office prevailed; but from the beginning of the present century this sum was granted from the private funds of the College.

From 1654 to 1692 the Fraternity, or College, occupied Trinity Hall; for some years after the latter date they occupied Sir P. Dun's house on the Inn's-quay; at a subsequent period their meetings were held at a house in Sackville-street, and later in the house of the President for the time being. As already stated, they occupied part of Sir P. Dun's Hospital from 1818 to 1864, when they removed to the new buildings in Kildare-street. The foundation stone of these buildings was laid on Monday, 7th of July, 1862, by the late Earl of Carlisle, when Lord Lieutenant, during the presidency of Dr. (now Sir Dominic) Corrigan, to whose exertions the erection of these buildings is in a great measure due. The last meeting in the Convocation room at Sir P. Dun's Hospital was held on Friday, the 1st day of July, 1864; and the first meeting was held in the new buildings on the Tuesday following.

The bi-centenary of the incorporation of the College will fall on the 8th of August, 1867.

BOOKS RECEIVED, AUGUST, 1866.

1. Lectures on mental diseases. By W. H. O. Sankey, M.D., &c. London: Churchill, 1866. 8vo, pp. 281.

2. A handy book of ophthalmic surgery, for the use of practitioners. By J. Z. Laurence, F.R.C.S., &c., and Robt. C. Moon, with numerous illustrations. London: Hardwicke, 1866. 8vo, pp. 160.

3. On inhalation as a means of local treatment of the organs of respiration, by atomized fluids and gases. By Herman Beigel, M.D., L.R.C.P.L., &c. London: Hardwicke, 1866. 8vo, pp. 200.

4. Surgical appliances and minor operative surgery. By Thos. Annandale, F.R.C.S., Edin., &c. Edinburgh: Mac-lachlan and Stewart. Fcap, pp. 246.

5. A practical treatise on the diseases of the testis, and of the spermatic cord and scrotum. By T. B. Curling, F.R.S. Third edition, revised and enlarged. London: Churchill, 1866. 8vo, pp. 609.

6. Dictionnaire encyclopédique des sciences médicales. Tome 4^{me}. Première partie. Amp-Ane. Paris: Asselin, 1866. 8vo, pp. 384.

7. On the curability of certain forms of insanity, epilepsy, catalepsy, and hysteria, in females. By Baker Brown, F.R.C.S., &c. London: Hardwicke, 1866. Post 8vo, pp. 85.

8. On the safe abolition of pain in labour and surgical operations by anesthesia with mixed vapours. By Robt. Ellis, Surg. Accoucheur to the Chelsea, Brompton, and Belgrave Dispensaries. London: Hardwicke, 1866. Crn. 8vo, pp. 80.

9. Cholera in its home, with a sketch of the pathology and treatment of the disease. By John Macpherson, M.D. London: Churchill, 1866. Post 8vo, pp. 155.

10. Eighth annual report of the general board of commissioners in lunacy for Scotland. Blue book, 1866. 8vo, pp. 266.

11. On the use of the sphygmograph in the investigation of disease. By B. W. Foster, M.D., &c. London: Richards, 1866. Fcap, pp. 32.

12. On dropsy, and its connexion with disease of the kidneys, heart, lungs, and liver, as well as on some other diseases of those organs without dropsy. Illustrated

by numerous drawings from the microscope. By W. R. Basham, M.D., &c. Third edition, enlarged, revised and rearranged. London: Churchill, 1866. 8vo, pp. 487.

13. Brief remarks on cholera, being the result of observations during the two last outbreaks of cholera in England, and an attempt to advance a theory of that disease which shall lead to a more consistent method of treatment, to which is added a short table of practical rules for general use during an epidemic. By Robt. J. Spitta, M.D. London: Churchill, 1866. 8vo, pp. 15.

14. The royal London ophthalmic hospital reports and journal of ophthalmic medicine and surgery. April, 1866. London: Churchill.

15. Gout and rheumatism in relation to disease of the heart. By A. W. Barclay, M.D., &c. London: Churchill, 1866. Post 8vo, pp. 214.

16. Defects of sight and hearing, their nature, cause, prevention, and management. By J. Wharton Jones, F.R.S., &c., &c. Second edition. London: Churchill, 1866. Fcap., pp. 168.

17. The common nature of epidemics, and their relation to climate and civilization. Also remarks on contagion and quarantine. From writings and official reports by Southwood Smith, M.D., &c. Edited by J. Baker, Esq. London: Trübner, 1866. 8vo, pp. 130.

18. The physiological anatomy and physiology of man. By R. B. Todd, Wm. Bowman, and Lionel Beale. A new edition by the last-named author. London: Longmans, 1866. 8vo, pp. 155.

19. Notes on health in Calcutta and British emigrant ships, including ventilation, diet, and disease. By W. H. Pearse, M.D. London: Churchill, 1866. Fcap., pp. 160.

20. The principal baths of France considered with reference to their remedial efficacy in chronic disease. By Edwin Lee, M.D., &c. Fourth edition. London: Churchill, 1866. Fcap., pp. 154.

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PART I.
ORIGINAL COMMUNICATIONS.

ART. VII.—*On Amputation at the Hip-joint.* By RICHARD G. BUTCHER, Esq., President of the Royal College of Surgeons in Ireland; Honorary M.D. of the University of Dublin; Associate-Fellow of the College of Physicians of Philadelphia; Surgeon to Mercer's Hospital, Dublin; and Lecturer on Clinical Surgery.

ENORMOUS OSTEO-SARCOMATOUS TUMOUR INVOLVING THE LEG AND THIGH EXTENSIVELY; AMPUTATION AT THE HIP-JOINT.

AMPUTATION at the hip-joint is deservedly ranked amongst the most formidable operations in the whole range of surgery. The risk from hemorrhage, primary and secondary, the peril from shock, the danger from phlebitis, from blood-poisoning from so extensive a suppurating wound, make the surgeon pause ere he adopts it as a last and necessitous resource for the salvation of life. When once the propriety of the measure is admitted and settled upon, no time should be lost; hesitation and delay should never take place, lest the favourable time should pass away, the operation be performed too late, and the patient's doom more quickly sealed; besides, by such imprudence, practitioners may be intimidated, science retarded and severely injured.

On the 20th of August, 1819, the late Mr. Richard Carmichael removed the lower extremity at the hip-joint, for a large osteo-sarcomatous tumour, in the Richmond Hospital. On the fifth day the patient expired.

This is the first instance in which the operation was performed in Ireland—forty-seven years ago—and from that date until the present it has never been repeated. I am therefore anxious to draw attention to the operation, and lay before my professional brethren the history of a case upon which I recently operated, with such observations as I think best suited to the subject.

The necessity for amputation has always existed, and ever will continue, so long as the destructive effects of injuries and diseases of the limbs cannot be obviated in any other manner. Happily for science and humanity conservative surgery has done much in the present century to lessen mutilation and, at the same time, to preserve for the sufferer a limb far better for the performance of its functions, be it the upper or lower extremity, than any mechanical contrivance, no matter how admirably devised and executed, could possibly accomplish; and the surgeon who would now amputate the lower extremity for disease of the knee-joint, when excision was applicable, or who would lop off the upper extremity for disease of the elbow-joint, when excision was suitable, in my mind should be considered just as censurable as the surgeon who would peril a patient's life by tying the femoral artery in an ordinary case of popliteal aneurism, without first having submitted the patient to a trial of the treatment by compression, so carefully taught and inculcated in the truthful statements published by the late Dr. Bellingham.

There are certain cases, however, where amputation affords the only chance of saving or prolonging life; amongst these may be classed, where incurable disease seizes upon an extremity, and involves its destruction to a greater or less extent, far up, near to its articulation with the trunk; then, be it upper or lower extremity, as a *dernier ressort* it must be taken away. Mr. Hennen, in his valuable work on military surgery (p. 251, Ed. 2.), makes this excellent observation, founded on the purest humanity, and justified by the soundest professional principle, that *to save one limb is infinitely more honourable to the surgeon than to have performed numerous amputations, however successful*; but it is a remark, notwithstanding its quaintness, fully as true, that “it is much better for a man to live with three limbs, than to die with four.”

Mr. Samuel Cooper justly states that the very idea of this formidable operation—amputation at the hip-joint—for a long while checked the hand even of the most ready advocate for the amputating knife, and every mind shuddered at so extensive a mutilation—yet it could not be denied that the chance of saving life occasionally depended upon a submission to the greatest temporary suffering, and that without the most cruel of sacrifices the preservation of the patient was totally impossible. Dreadful as amputation at the hip appeared, both in respect to the magnitude of the part of the body to be removed and the extent of the wound caused by such removal, the desperate nature of some cases at length began to incline surgeons to view more dispassionately a scheme at which the mind at first naturally revolted. But yet, to show how the love of life—impressed, I believe, on man as his great safeguard—will protect him and regulate his actions to insure that security, I will just describe what Morand, the great French surgeon, the earliest practitioner who made this severe operation the subject of attention, has stated in his work on surgery (*Opusc. de Chir. p. 183*). He reminds the reader that in the Hotel des Invalides, at Paris, mutilated objects are in recollection who had lost all their thighs and arms, so that, unless assisted, they could not stir, and it was necessary to feed and wait upon them like new-born infants. Let it not be wondered at, then, how some are influenced by a relish for life. This same great surgeon, it would appear, was the first who ventured to express an opinion on the possibility of the success of the operation. This question was taken up with much assiduity by the Academy of Surgery, and a favourable report was returned by Ledran and Guerin. It became then a prize question in the French Academy, at the instance of Morand, in 1756, and again in 1759; none of the essays on the first occasion were found worthy of the prize, which on the second was awarded to Barbet, who forcibly advocated the propriety of the operation, and pointed out the cases to which he considered it rationally applicable. If, for instance, a cannon ball, or any other violent contusing cause, had carried off or crushed the thigh, so as to leave only a few parts to be cut to make the separation complete, he thought a surgeon ought not to hesitate about doing it. Other instances are cited, but I do not think they bear upon the question—the propriety of the operation, with its attendant dangers; the cases portrayed cannot be considered as amputation at the hip-joint. The proceeding, as detailed by this author, seems to me to have no analogy at all to

the bloody operation of taking the thigh bone out of the socket; it is quite a different thing when the operator has to cut through parts which bleed profusely, and are endowed with life and sensibility. Though the operation was first considered in France, and numerous essays were written for the several prizes offered year after year, it is a remarkable fact in the history of surgery, that the operation should have been first put in practice in England—the operator being Mr. Henry Thompson, surgeon to the London Hospital. Larrey tells us he performed the operation twice in Egypt, and once while he was serving with the French army on the Rhine. He was encouraged to make these attempts to save his patients by the consideration that he had already preserved some lives by amputating either both thighs, both legs, or both arms, or removing the humerus at the shoulder-joint. Larrey has also the merit of having first done the operation in this description of cases in which (with the exception of bad cases of necrosis, or of incurable disease of the higher part of the femur) it is more decidedly proper than any other, viz., gunshot injuries of the head, neck, and upper parts of the femur, with or without injury of the femoral artery, or, when the limb has been carried away by a shell or cannon ball too high up to admit of amputation in the ordinary manner. However, he also regards as fit occasions for amputation at the hip-joint, circumstances in which from gunshot violence the limb is seized or threatened with gangrene nearly up to the hip.—*Mem. de Chir. Mil.*, T. ii., p. 185.

Severe as the operation is, Larrey contends that if it ever proves the means of saving lives which are in danger it is an act of humanity. After this period many successful cases followed—by Millingen, Brownrigg, Guthrie, Delpech, Orthon, Mott of New York, Wedomyr, Syme, Brice, Sir A. Cooper, and others; and taking the converse, the operation, even in the course in which it is indicated, has failed in the hands of some of the most eminent surgeons, amongst others Sir A. Cooper, Sir B. Brodie, Dupuytren, Mr. Guthrie, Baron Larrey, Blick, Cole, Delpech, Dieffenbach, Emery, Gensoul, Clot-Bey, Roux, Carmichael, and Pelikan of Wilna.—*Cyclopædia of Practical Surgery*, p. 182.

The successes of the operation in later years, I am persuaded, must in a great measure be attributed to the effects of chloroform, in averting the shock, and likewise to a careful selection of the cases operated on; no one now would think of removing the limb from the socket in scrofulous disease of the joint, yet it might be perfectly justifiable to excise the diseased head of the bone.

Mr. Erichsen (*Science and Art of Surg.*, 4th Ed., 1861, p. 59.), availing himself of the labours of Dr. P. Smith, and also of those of other writers, has given the result of one hundred and twenty-six cases of this operation, of which seventy-six proved fatal; of forty-seven cases in which it was performed for injuries, thirty-five died. According to Mr. M'Leod, of the ten cases that occurred in the Crimea, not one recovered. Mons. Legoust, who served with the French army in the Crimea, collected thirty cases of this amputation, primarily performed for gunshot wounds, which all terminated in death:—"Some died during the operation itself, others soon after they had been carried to their beds, and all within ten days, except two cases mentioned by Larrey, one of whom lived twenty-one, and the other thirty days."

I find in the very beautiful book issued from the War Department, Surgeon-General's Office, Washington, 1865, (at page 48,) the following satisfactory report:—"At the commencement of the war the uniform fatality of amputation at the hip-joint in the Crimean war was impressed upon the minds of surgeons, and many believed that the operation should be discarded altogether; still it has been occasionally performed, and several lives have been unquestionably saved by it." Then follows the details of a very interesting case of primary amputation, performed, April 29th, 1863, by Surgeon Shippen. A letter, dated January 12th, 1865, was read from the patient, and represented him as in excellent health and spirits; a beautiful photograph, taken from the man a year after the operation, accompanies this description. Another successful primary case, by Dr. Fenner, is likewise mentioned in this valuable book; operation performed twenty-four hours after receipt of the injury. A table shows a return of twenty-one cases—nine were primary, and twelve were secondary operations.

The conclusions arrived at by the writer as to the advisability of early amputation at the hip-joint, after gunshot injury, is thus put. There seem to be but three conditions under which early amputation at the hip-joint is admissible in military surgery, viz.: when nearly the entire thigh is carried away by a large projectile; when the totality of the femur is destroyed by osteo-myelitis; and, possibly, when, with comminution of the upper extremity of the femur, the femoral vessels are wounded.

The experience of M. Jules Roux, in the Italian war, seems to prove conclusively, that secondary amputations at the hip-joint are less dangerous than primary ones; he has practised it four times,

and in all cases with success. Dr. Gross, in his magnificent work on surgery (Gross's *System of Surgery*, Vol. II., p. 1044, 3rd Ed.), cites a most interesting case, operated on successfully by Professor Pancoast, on account of a large encephaloid tumour of the upper part of the thigh; patient thirty-eight years old. The operation was performed in the Pennsylvania Hospital, in June, 1860; pressure was made upon the abdominal aorta, by means of a tourniquet encircling the body at the umbilicus.

Professor Gross likewise gives the details of a case upon which he successfully operated; the patient, although exceedingly anemic and exhausted at the time, recovered without any untoward symptoms. Engravings from photographs of both these cases, after cure, are given, and are in every way highly creditable to surgery.

In the *Lancet*, Vol. II., 1864, p. 551, Dr. Frazer, of Calcutta, gives the details of a successful case of this chief of amputations which was, however, secondary to an amputation of the thigh twelve days previously; this was stated to be the second successful instance in India, the first having occurred to Mr. Wigstrom of the 24th Dragoons; the details of this case will be found in the *Lancet* for 1850. In the *Lancet* for June, 1865, a case is recorded where Mr. Lane, of St. Mary's Hospital, operated on a woman, aged twenty-one, for malignant disease of the thigh, of rapid and painful growth; she recovered from the operation, but death followed in eight weeks afterwards, from pus in the femoral artery. In this same number of the *Lancet* we have the following table (page 303) showing the results of the operations in the metropolitan hospitals. All severe injuries requiring it, terminated fatally.

The operation has been performed unsuccessfully by Mr. Wheatcroft of Cannock, on a woman, aged forty (*Lancet*, May 21st, 1853); fatally also by Mr. W. Swain, of Devonport, on a man aged thirty-six years, for encephaloid tumour of the left thigh (*Lancet*, May 27th, 1865); and at the Jersey Hospital, successfully, by Mr. Godfray, on a boy aged fifteen, for extensive necrosis of the femur. I have pleasure in drawing attention also to Mr. J. J. Gamgee's successful case operated on in the Birmingham Hospital, and published in a separate memoir, illustrated with four very beautiful photographic pictures, and reviewed in the *Lancet*, November, 1865; and it should not be forgotten that Mr. Sands Cox's interesting case and essay on the same subject, emanated from the same town thirty years before.

Table showing the Results of Amputation at the Hip-joint, in the London Hospitals.

No.	Reference to the <i>Lancet</i>	Date of operation.	Operator	Hospital	Sex	Age	Nature of Disease	Result
1	Vol. I., 1857, p. 31	1852	Mr. Hancock	Charing-cross	M.	52	leg, and	Death immediately afterwards from shock.
2	Vol. I., 1853, p. 405	1853	Mr. C. Guthrie	Westminster	M.	50		Death on second day, from exhaustion and hemorrhage.
3	Vol. I., 1854, p. 442	1854	Mr. Adams	London	F.	26	Fibro-cystic disease of leg and thigh	Death on twelfth day, from diarrhoea.
4	Vol. I., 1855, p. 363	1855	Mr. Erichsen	Univ. College	M.	26	Severe compound fracture of thigh, high up	Death two hours after.
5	Vol. I., 1857, p. 444	1855	Mr. Tatum	St. George's	M.	17	Medullary cancer of femur	Recovery.
6	Vol. I., 1855, p. 6	1855	Mr. Curling	London	F.	40	Medullary cancer of thigh	Recovery; death in ten months, from internal disease.
7	Vol. I., 1857, p. 31	1855	Mr. Hancock	Charing-cross	M.	24	Extensive disease of thigh and entire femur	Recovery.
8	Vol. I., 1857, p. 343 and 386	1857	Mr. Stanley	St. Barthol.	M.	52	Medullary cancer of femur	Death two hours after, from hemorrhage.
9	Vol. II., 1857, p. 445	1857	Mr. Lane	St. Mary's	M.	29	Extensive injury to thigh, groin, and perineum	Death in five hours, from shock.
10	Vol. I., 1860, p. 319	1860	Mr. Hancock	Charing-cross	M.	35	Disease of stump in thigh, after two previous amputations	Death in twelve hours.
11	Vol. I., 1865, p. 651	1862	Mr. Lane	St. Mary's	F.	21	Malignant disease of thigh	Recovery; death in eight weeks, from pus in the femoral artery.
12	Vol. I., 1865, p. 652	1862	Mr. P. Young	St. Mary's	M.	21	Extensive fracture of both limbs	Death, a few hours after, from shock.
13	Vol. I., 1865, p. 652	1862	Mr. Wells	Samaritan	F.	26	Malignant tumour of the thigh	Recovery; death a year after, from cancer.
14	Vol. I., 1865, p. 566	1863	Mr. Lane	St. Mary's	M.	17	Malignant disease of femur	Recovery; death in two months, from chest disease.
15	Vol. I., 1865, p. 566	1863	Mr. Curling	London	M.	7	Necrosis of shaft, head, and neck of femur	Death in five weeks.
<i>I shall add to this Table the following three cases, which occurred in the London Hospitals, and were reported in the Lancet.</i>								
16	Vol. I., 1866, p. 222	1865	Mr. Erichsen	Univ. College	M.	27	Encephaloid tumour of the femur	Immediate effects of operation recovered from; wound still discharging when report closed.
17	Vol. I., 1866, p. 367	1865	Mr. Holmes	St. George's	F.	36	Recurrent fibroid disease of thigh	Recovery.
18	Vol. I., 1866, p. 386	1865	Mr. Lee	St. George's	M.	14	Disease of hip-joint and pelvis	Recovery.

Table showing the Results of the Cases of Amputation at the Hip-joint, which have occurred at Philadelphia.

No.	Operator	Year	Sex	Age	Disease	Interval between First Appearance of Disease or Injury and Operation	Operation	Result	Cause of Death	Remarks
1	Duffy	1840	Female	6	Morbus coxarius	—	—	Recovered	—	Seventeen years after—
2	Pancoast	1860	Male	32	Medullary sarcoma	One year	Integumentary flaps and circular of muscles, accupressure and ligatures	Recovered	—	Alms House. Stump well. Died two years after-wards from a return of the disease in the lymphatics adjacent to the stump. Remained well, 1864.
3	Gross	1862	Female	12	Loss of integument, and suppurating following a burn	Fifteen months	—	Recovered	—	Lived twelve hours.
4	Hewson	1864	Male	6	Railroad injury	Within twenty-four hours	—	Died	Renewed shock	Remains well.
5	Pancoast	1866	Female	30	Huge osteo-chondromatous tumour	Seven years and eight months	Integumentary flaps and circular of muscle	Recovered	—	Remains well.
6	Gross	1866	Male	64	Encephaloid	Four years	Integumentary flaps and circular of muscles	Recovered	—	Remains well.
7	Hewson	1866	Female	28	Enchondroma	Several years	Flap operation, accupressure	Died	Shock	Lived fifteen hours.
8	Packard	1866	Male	31	Re-amputation of stump of diseased bone	Five months	Flap operation	Recovered	—	Remains well.
9	Morton	1866	Male	21	Re-amputation for osteo-myelitis	Sixteen months	Integumentary flaps and circular of muscles	Recovered	—	Remains well.
10	Agnew	1866	Male	43	Disease of femur and soft parts following a gunshot wound	Three years	Integumentary flaps and circular of muscles	Died	Hemorrhage from the femoral artery 12 days after operation	Lived sixty hours
11	Forbes	1866	Male	—	Diseased femur following gunshot wound	—	Integumentary flaps and circular of muscles	Died	—	—

In the *American Journal* for the month of July, 1866, will be found an interesting paper by Dr. Thomas Morton, one of the surgeons to the Pennsylvania Hospital, with an account of a case in which he amputated successfully at the hip-joint; and in this memoir he likewise details the histories of all the cases, eleven in number, which have occurred in Philadelphia.

The table (on page 304), which is appended to his observations, is very interesting, conveying much information, and showing at a glance the results of these several amputations at the hip-joint.

I am not aware that any recently published case has escaped my notice, so now shall detail the history, operation, procedure, and management of one recently under my charge in Mercer's Hospital.

Enormous Osteo-Sarcomatous Tumour Involving the Leg and Thigh Extensively; Amputation at the Hip-Joint.

Mary Keogh, aged fifty-one years, admitted to Mercer's Hospital, in November, 1856. Her occupation was that of a servant, being employed chiefly in scouring out rooms. More than four years and a half before her admission on the above date, she struck her right knee forcibly upon a stone step, having slipped when carrying up a pail of water; the pain was very severe in the stricken part, on the inner surface of the head of the tibia—so acute that she became weak, and fainted; she was compelled to stay quiet for some days, but within a fortnight resumed her ordinary occupation. When in the kneeling position she constantly felt uneasiness; and, though but resting little weight upon the part, yet she was very sensible of its inability to bear the burden equally as the other; and so struggled on, occasionally working, always pained, often laid up altogether, and totally incapacitated from either kneeling or walking; slowly and gradually an enlargement of the upper part of the tibia, on its inner surface, attracted attention; an oblong defined swelling, not discoloured or painful, except when pressed forcibly upon; in its most elevated part it did not exceed three-quarters of an inch, and gradually fined off to its circumferential limits. Shortly after this, pain left the part altogether, confidence in the movements of the limb was regained, and the patient followed her former avocations uninterruptedly for many a month—I may even say years; she was industrious, and constantly employed, and though occasionally pained a little in the part, yet never to such an extent as to interrupt her engagements, or confine her to bed.

Four months before the date of the patient's reception into the hospital, she slipped in coming down stairs, and was precipitated to the next landing, where she fell heavily, the right limb being violently twisted under her; after this accident she was unable to move the leg, or rest in the least degree her weight upon it, and had to be carried to bed, where she was treated for a period of six weeks; at the end of this time she was sufficiently recovered to get up and resume her business and duties. It was remarkable at this time that the swelling on the inner side of the tibia was not increased or rendered more sensitive, in consequence of the recently-sustained injury; for though the enlargement upon the tibia participated at first in the general uneasiness around, yet the joint suffered far more by the straining, stretching, and twisting of its ligaments; pain and tenderness forsook the swelling long before it was removed from the ligamentous structure binding the bones; the tumour seemed to suffer more from contusion—the joint from being forcibly wrenched; again the patient went about so far improved, as to walk and to be able to flex the leg upon the thigh, its dimensions being in no part increased, and the enlarged part of the tibia not in the least rendered more prominent or sensitive upon pressure; again, the woman met with an accident three weeks before her coming under my care: she slipped off the curb-stone of the pathway, and fell with violence on the affected limb; the leg was checked in the fall, and turned outwards, the swollen part of the tibia coming in contact with the sharp edge of the curb-stone, she suffered very severe contusion, accompanied with acute pain and attended with faintness and vomiting; she had to be lifted from the ground and conveyed home. When she was laid in bed, the limb was stuped, leeches, and placed in the most favourable position, constitutional means were likewise had recourse to, and, by the efficient combination of local and constitutional measures, the early inflammatory symptoms developed in a marked way, even in a few hours, were subdued, but not removed altogether, and at this stage of the affection the patient was admitted to hospital. Her condition at this period was as follows:—She was emaciated and greatly depressed, suffering much pain at intervals, and chiefly the severest paroxysms at night; restless and agitated by day; appetite gone, and even a loathing to nourishment of every kind; pulse feeble, yet steady in its beat, but rapid, averaging about 94. The limb at this time was but little enlarged above the knee, and the knee but little participated in the swelling or expansion of the inner side of the upper part of the tibia; the

integuments over the diseased region were not at all discoloured, and the osseous tumour bore pressure with impunity all over its surface, except at one part, that where the injury was directly applied, in front and towards the articular surface of the bone; the patient could not, of her own accord, flex the leg upon the thigh, beyond a very obtuse angle, the position in which she endeavoured to keep it, as affording most ease and rest; however, I could perform flexion at the joint to fully a right angle without inducing pain; at this time the joint structures around were thickened, and the internal condyle of the femur considerably more prominent than the outer one, yet pressure could be borne here also without eliciting much suffering; the circumference of the limb, both beneath, around, and above the joint, exceeded the sound one by three, two, and one inch, in each of these respective positions. At this time every means were employed to allay pain, to procure rest, to build up the enfeebled health, and, if possible, to check the progress of the affection. The case, however, went on, being but little restrained by treatment; this became quite apparent in about a fortnight, the additional enlargement being most manifest over and around the site of the original tumour upon the tibia; the pain referred to this part was most acute; no local application seemed to have much effect in either mitigating or lessening its duration; the most powerful sedatives—watery extract of opium and extract of belladonna, made into a thick paste, separately or together, and applied over the surface, soon lost their effect in diminishing the burning pain when at its height; another peculiar feature in the pain was its absence, sometimes for short periods altogether, when suddenly it would set in in its most aggravated form. The nature of the tumour, even at this early time, was accurately diagnosed, and lectured upon to a large class of students, and at this period the proposal of amputation at the thigh was urged upon the patient, but to this she would not listen.

Several weeks passed on, and the tumour steadily enlarged in all directions; the pain seemed less acute, often absent for many hours; with this release from intense agony, her general health assumed a better aspect, she ate considerably more, she often had quiet and refreshing sleep for some hours at a time, induced no doubt by the exhibition of sedatives, but only in such doses as that failed entirely at an earlier period of the case in suspending pain or procuring rest; so rapidly did the tumour increase, that from day to day the augmented bulk was perceptible to all; it seemed indiscriminately

to seize upon all surrounding structures; the ham soon became filled up, and almost bursting; the tension in this locality was extreme, and the strained integuments bright and shining; the whole leg and foot became enormously swollen, with hard, firm, œdema, and so great and unmanageable by its bulk as absolutely to cause extensive ulceration on its outer side, from its weight when supported and resting on pillows; as the disease progressed, no other position but that adverted to could be endured, so ponderous had the entire member become that the patient had to lie partially turned upon the right side, the limb partly flexed, the thigh upon the pelvis, and the leg upon the thigh, all equally sustained upon pillows; this was the constant attitude maintained up to the period of operation. At this date, a few days before operation, the measurements of the limb were as follows:—

1st. Around the knee, at the angle of flexure, thirty-five inches.

2nd. Around the thigh, at its centre, eighteen inches.

3rd. Around the leg, just below the joint, twenty-one inches.

4th. Around the leg, just at the calf, twenty inches.

5th. Around the ankle, thirteen inches.

The accompanying beautiful plate, executed by Foster from a photograph by Robinson, will give, at a glance, the proportions of the limb; it was sustained in the elevated position delineated, by a bandage tied above the knee, and fastened to a fixed support, on the other side of the patient.

So completely did the weight of the tumour chain the patient to the one position, that it was with great difficulty she was kept from stripping; the constant application of nitrate of silver, and of strong oak-bark washes, with alum and tannic acid, together with well adapted cushions, prevented so grave and distressing a complication; and now her pains became more marked, developed, and persistent, independent altogether of her movements being restricted—and faster and more rapidly she was overburdened and surrounded by distress, without any possible hope of amelioration or repose; and so, now, she fixed her whole hopes of relief upon operative interference—that refuge, which had been at an earlier period so pressed upon her for safety—when safety and security for a time at least was surely certain—for the same risks did not then hang around the case, and amputation might have been performed at the middle of the thigh, or as I should prefer to advocate higher up, with every prospect of success and recovery; but in the condition of the patient in this last stage, now, at this period, when she would only assent to

operation, when she absolutely pressed hard for even a chance of escape from her miseries, and her sufferings, and her impending fate; no operation was applicable, but the removal of the entire limb—the thigh bone, from the socket—the separation of the whole polluted member from the trunk—for the soft parts were contaminated, infiltrated, and diseased, far up beyond the knee; and the articular surface and lower part of the femur were extensively implicated; the consideration of these well ascertained facts settled the question as to disease of the thigh. The groin was free from any glandular enlargement, there was no puffing, or more correctly, infiltration of the integuments here, or morbid state of the veins or lacteals of the region. The chest was subjected to the most careful stethoscopic examination; the lungs were remarkably healthy, resonant, and clear throughout; the heart normal in its proportions—vigorous, steady, and equal in its beat; the abdominal cavity was likewise submitted to careful inquiry, and no change could be discovered either in the solid or soft viscera; and, I need scarcely add, among the former the liver was subjected to the strictest scrutiny and most delicate manipulation. As no aberration from a healthy state in any of these organs or regions could be detected, and as the woman's whole heart and confidence were centered in the expediency of operation, I willingly assented to her wishes, and, after a deliberate consultation with my able colleagues, determined to remove the limb from the hip-joint on the following morning.

February 12th.—The patient was brought down from the ward in which she lay to a small room beside the operating theatre, and used for the reception of patients after being operated upon.

I shall digress for a moment while I describe the arrangements I conceived necessary to be made in the theatre:—First, as to the table, it should be higher than for ordinary operations—I conceive about four feet high; such an elevation will afford the surgeon great facility in securing the vessels in the posterior flaps, and rapidity in this part of the procedure is of the utmost importance, and should never be lost sight of. Should the operator find it inconvenient to transfix the limb with accuracy, sustained at such a height, he should raise himself by standing on a solid wooden support, so that he should have full command over the parts, and not be restrained or checked in using the knife according to his will and judgment; again, the table should be narrow, so as to allow the operator and assistants to be near their work, and so act vigorously, and in concert together; a folded blanket should take the place of

the mattress, and be evenly turned back upon itself, at the end of the table, so as to present a well-defined edge upon which the buttocks of the patient will rest without a chance of slipping; and a couple or three pillows will be requisite to sustain the head and shoulders of the person. Matters being so prepared, the patient was placed under the influence of chloroform before she was brought into the theatre, and placed upon the table, perfectly calm and insensible; she was brought well down to the end, so that the buttocks projected slightly over it; the affected limb being the right one, the assistants were placed, as follows:—One to make pressure on the femoral artery, standing on the left of the patient; one to take charge of the anterior flap, standing on the left of the patient; one to hold back the left leg, and to prevent the pelvis slipping forwards or to either side, standing on the left of the patient; two kneeling, one on either side, to grasp the posterior flap, as the limb was cut free; and one to support the diseased member, and to move it as required in the different steps of the operation; an assistant was also placed to restrain back the shoulders, and so effectually to guard against the patient gliding downwards and slipping off of the end of the table; so that, according to my arrangement, six assistants are required, independent of one, who must continue to administer the chloroform. Now, let me not be misunderstood, because I recommend so many assistants, and so accurately place each, that I would not proceed to execute the operation with half the number if the exigencies of the case demanded it, and that such confident support could not be procured; I have laid down the rule according to my judgment, when it can be carried out, and must of course leave room for the exceptions which will sometimes occur, and must be acted upon, and with great propriety; sooner than all hope in relieving the sufferer should be abandoned, I have before now with one assistant successfully amputated the right arm, high up, under great disadvantages, grasping the limb myself and suppressing hemorrhage with the left hand, while my assistant steadied the diseased member during its severance from the body and tied the vessels as I drew them out by the forceps or tenaculum. The assistants being arranged as already described, the limb was elevated high up for a few minutes, to diminish as much as possible its contained quantity of blood, and lessen the inferrent current; soon then a bandage was rolled very rapidly from the foot upwards, and applied very tightly above the centre of the thigh, as a preventative to guard against a rush of blood from below during the formation

of the flaps; then the limb was depressed to a suitable angle, the thigh slightly flexed upon the pelvis, so as to permit the fair travel of the knife behind the vessels. Standing on the right side of the patient, well over her—and pressure being *now* safely made effective on the main artery, just above the pubes, the assistant using a large key, well guarded with rolls of flannel round its handle, for the purpose—I entered a long double-edged knife, fully twelve inches in the blade, and three-quarters of an inch in depth, midway between the spine of the ilium and upper edge of the great trochanter, about three-quarters of an inch nearer to the former, the flat surface of the blade looking forwards, and a little downwards, parallel with the plane of the muscle; striking the knife steadily inwards, till it touched lightly the neck of the bone, high up, and so accurately giving evidence of its position, it was glided over the bone, and so safely beneath the great vessels; still onward the blade was passed till it appeared on the inner side of the thigh, and protruded about an inch and a-half between the point of junction between the descending ramus of the pubes, and the tuberosity of the ischium; the knife was then carried rapidly downwards, close to the bone, for four inches, and then obliquely forwards, cutting loose a flap about six inches long; as the knife made room I passed the fingers of my left hand beneath the flap, making steady pressure, and followed the knife until about to turn it forward to sever the femoral artery, and cut its way out, thus making certainty of effective pressure; the flap being so formed the assistant took charge of it, and in a similar way commanded its vascular supply. During the formation of this anterior flap, while the knife was traversing as it were in a semicircular course, the limb was not only slightly flexed but a little rotated inwards; after this the limb was forcibly abducted, and at the same time depressed. Then the heel of the knife was laid determinedly upon the capsule, over the neck of the bone, and its head unhesitatingly started from the socket of the acetabulum, and then as rapidly as thought, the blade of the instrument was carried behind the bone and great trochanter—severing the round ligament, and completing, by a slight downward movement and then backward, the posterior flap; as the knife formed it, and it became detached, it was firmly grasped both within and without by the assistants kneeling on either side. When the head of the bone was disarticulated, and the knife rapidly passing behind, the assistant who held the limb depressed the knee, and lifted at the same time the head of the bone forwards and

upwards, and so facilitated in a marked way the transit of the instrument in the course sought for and prescribed. My able assistants, Drs. Jameson, Bevan, Ledwich, and Mason, were so experienced and steady in all their duties and movements that they permitted me to execute the detailed steps of this operation, and so sever the limb from the body in the period of eleven seconds. The next step to be considered was the securing of the vessels. Now, the pressure upon the femoral trunk was so admirably and efficiently made by Dr. Jameson, and all the vessels of the anterior flap so perfectly controlled, that I at once set about securing them in the posterior; in this dissection, too, the hands of the assistants fulfilled their work: three large trunks were secured of the gluteal artery, one of them in the outside angle, remarkable for its size, retracted deeply, and had to be dissected out from its bed, and tied; the sciatic artery was likewise remarkable for its size, and carefully disentangled from its important nervous relations—and secured; four large arteries were drawn out, and tied, towards the inner side of the flap. During all this careful procedure not a spoonful of blood was lost, so efficiently did the manual pressure exert its control; finger after finger could be raised up, each giving special information of where a vessel demanded vigilance, and required to be ligatured. All behind being fairly secured, and a large sponge wrung out from hot water applied steadily to the surface, I proceeded to deal with the anterior flap, and first tied, with a strong silken ligature, the femoral artery; and afterwards three large vessels, branches of the profunda—and a fourth which I considered to be the obturator; three vessels of smaller size were likewise tied; though numerous the arteries requiring to be tied, it was very remarkable how readily the task was accomplished, and during the whole operation scarcely an ounce of blood was lost; in fact it was a bloodless operation so far as any hemorrhage on the cardiac side was concerned; of course some flowed from below the selected points for division, independent of the strenuous efforts which had been made to unload the doomed member of some of its contents, by maintained elevation, previous to operation, and then its sudden forcible constriction after the main supply was cut off by pressure at the groin, and before the knife was applied. During all this time the patient lay perfectly still and quiet under the influence of chloroform, and I let her rest upon the operating table for ten minutes, warm sponges being assiduously applied throughout the wound. At the end of this time reaction was fairly established,

and all pressure removed from the arterial supply to the stump, and as no vessels showed a disposition to bleed, I had the patient removed to her bed, in the small ward, beside the operating theatre, and when comfortably laid upon it, the flaps were evenly supported thus: a small soft pillow was laid beneath the posterior one, upon which rested a large piece of oiled lint, this pillow was so gradually insinuated behind that it evenly supported steadily forwards and upwards the pendent part; the anterior flap was gently turned upwards, and being of considerable length it rested so without constriction; in this condition the surfaces were left to glaze. During all this latter arrangement the patient was quite conscious; she was not aware that the operation had been performed, and never recollected having suffered any pain; it certainly was a wonderful sight to witness this creature, after so formidable, so terrible an operation—calm and undisturbed, and free from suffering—speaking cheerfully—the effects of the chloroform having entirely, at this time, passed away; she got wine liberally, and a full opiate; twenty drops of Battley's sedative.

At 4 o'clock, p.m., assisted by Drs. Jameson and Bevan, I proceeded to dress the wound; and I may now remark at this hour, six after the operation, the patient had well rallied; the pulse being up and steady, 96 in the beat; heat of skin healthily generated; the tone and volume of the voice good; and the patient, on being interrogated, stated she felt more comfortable than she had been for several months before, and was quite cheerful. I may remark that the stump lay upon the pillow, sustained just as we had left it; there was no tremulousness or wavering spasm of the flaps to distort it from its place; there had been no weeping of blood, and the entire surface of the flaps was well glazed; this favourable state of the parts may be assigned to wine and opiates, which she had freely partaken of, and to the occurrence of some sleep which had been early induced by opium. The precaution taken of having interposed oiled linen between the under flaps and the pillow upon which it rested, and between the integuments of the upper one and the surface upon which it lay—the base of itself and the lower part of the abdominal wall—cannot be over-estimated, as facilitating the ready approximation of the surfaces without violence; the withdrawal of the oiled linen at once set free the parts, and so allowed them, by the greatest gentleness, to be brought in contact. I may mention that the flaps were well glazed, and not a trace of oozing from them anywhere, and there were no dark clots in the interstices of contracted

fibres. Twelve points of the interrupted suture, silver wire, being employed, steadied all accurately together; the outer angle was not closed; a narrow strip of oiled lint was passed in deep, in this locality, to secure an outlet for any exuded fluids; a few broad straps of adhesive plaster were then gently applied from behind forwards, supporting a compress behind and before, bringing the parts accurately together, throughout the whole depth of the wound, surface to surface; a pillow was again placed beneath the buttock, with a gentle force, sustaining forwards and upwards, commensurate to counteract retraction, and the effects of gravity shortening the posterior flap; while the anterior one by this arrangement, this support, was effectually steadied, and its vascular supply neither impeded or interfered with; the several ligatures were brought out by the nearest route, through the edges of the wound, and protected by a few straps of plaster; the patient was dressed without pain of any amount, and without the least spasm or muscular twitchings, or effusion of blood; a good deal of this immunity from suffering must be ascribed to the gentle, efficient, and practised hands of those gentlemen who assisted me. Ordered some strong chicken broth to be given at intervals of every second hour, and a little cold brandy and water immediately after; also, every third hour, fifteen drops of Battley's sedative. 10 p.m.—Nothing could be more satisfactory than the condition of the patient at this time; she had refreshing sleep, often for an hour undisturbed, and partook freely of broth, wine, and brandy; the opiates allayed all pain; her pulse was only 96, steady in its beat; and with fair volume, she expressed herself as most thankful for the relief she experienced, and was quite bright, cheerful, and expectant of recovery; ordered to continue the beef-tea and chicken jelly whenever awake throughout the night, and the opiate every third hour.

February 13th, 9 a.m.—Had a remarkably good night, sleeping steadily and quiet; took a large quantity of nutriment and stimulants with eagerness and appetite; this morning she looks bright and happy, and says she is free from pain; occasionally a slight spasm in the stump, but it is not severe, and yields in a few moments; her pulse is steady, 96, and the beat of good volume; skin a little hot, but not burning; renal secretion abundant, but incapacity of voiding the vesical contents, effected by the catheter; no tenderness over abdomen, or in the right iliac region, upon pressure; the stump looks admirably; it is sustained and supported without the least

change from its position on last evening; it preserves a nice temperature, and along the whole track of the anterior flap the heave or beat of the femoral artery marks its course. A considerable quantity of sero-sanguineous fluid has escaped from the outer angle of the wound, and along the lint placed between its edges, to insure a ready egress; there is no puffing or tightness of the wound; the flaps seem just sufficiently full to show that the vascular supply is enough for repair, and the heat equivalent. I may here remark, nothing can be more dispiriting to the surgeon than to see, in twenty-four or thirty hours after amputation, a flaccid condition of the flaps; it shows a want of vitalizing power, and at the least is anything but salutary towards the speedy recovery of the patient; as to the treatment of the patient it practically speaks, too; such a condition demands a more rapid and abundant supply of stimulants and opiates; and I do, with confidence, say (not assert) from observation and experience, that I have seen many in this most critical position, rescued and saved by the means suggested; the two harmonize together—the one in arousing the sensibility of the nervous centres, and the heart's action, and so the proper supply of blood to the injured part; the other in quieting irritability and spasm of the muscles, and allaying pain. To continue wine, brandy, and nutriment, as already directed, and likewise the sedative draughts. 5 o'clock, p.m.—The patient is quite calm and free from pain, and has taken an abundant quantity of beef-tea and jelly; she has had quiet sleep since morning, and, on the whole, is wonderfully well; pulse 98; urine abundant, and still requires the introduction of the catheter for its escape; the stump lies perfectly at rest, the sedative influence of the opium averting all tendency to spasm; and it looks healthy, a quantity of watery fluid escaping at the outer angle. 9 o'clock, p.m.—No change to be recorded; nutriment, stimulants, and sedatives to be continued through the night.

February 14th, 9 a.m.—Slept well; countenance calm; free from all pain; pulse 98, regular and full in its stroke; tongue clean; bowels moved once, and urine flowed at the same time, by the efforts of the patient, and in considerable quantity. Readjusted the pillows under the stump, and gave the hinder flap a little more support forwards, by additional straps of plaster, those first put on having relaxed a little, from heat and moisture, owing to the persistent flow of pus and watery fluid through the outer part of the wound. Beef-tea, chicken jelly, wine, and opium to be given as before. 9 p.m.—Has had a quiet day, and consumed a large quantity of nutriment; it

tells well, for her pulse is stronger, more steady, and fuller in its beat, than before the operation; and altogether her appearance is far better; the anxious countenance, and the glazed eye are gone, and the expression is one of rest and peace; she speaks confidently of her recovery.

February 15th, 9 a.m.—The patient had quiet sleep, waking at intervals to take food and medicine; feels stronger; and took, for breakfast, some tea and toast with appetite; pulse 98, with full and steady beat. This day, the third after operation, I proceeded to dress the wound; the parts, being steadily supported behind and before, more particularly with a gentle force forwards; the adhesive straps were, with great caution, removed, and the oiled lint from the line of incision; to facilitate their removal a flannel stupe-cloth, wrung out of boiling water, had been laid over them for two hours previously; the external dressings being taken away, I very cautiously drew out the piece of lint laid within the outer angle of the wound; this came away readily enough, it being saturated with serum and pus from the beginning until now; a free escape of healthy pus followed, on pressure being made towards the bases of the posterior flap; three-fourths of the edges of the flaps were perfectly united, and lay beautifully together, no irritation or strain being in the least degree evidenced about the wire sutures, in any of their points; indeed this could scarcely be otherwise; the anterior flap was preserved in such full proportions; the parts all around the wound were well cleansed, and sponged with soap and tepid water; and the dressings readjusted as before. Caution was taken to sustain well forward the posterior flap, and compresses evenly applied, and gradually diminishing from behind forwards, laid on, to carry out efficiently this intention. The patient was next gently lifted from the bed, in the horizontal position, by a number of students, and fresh sheeting, pillows, &c., put upon it; upon these the creature rested most comfortably, expressing her gratitude for the relief she experienced. It was astonishing how little fatigue was consequent upon this trying task; the patient spoke well, and audibly; the pulse was not flurried; neither were the respirations embarrassed; she eagerly took a cup of strong beef-tea, and a draught of wine after, and then settled herself to sleep. 9 p.m.—Has had a good day, and seems much refreshed, after the removal of all soiled things from about her; the stump looks well, and she experiences no pain in it; purulent matter, healthy in character, is coming away abundantly from the outer angle of the wound. To continue broth, jelly, wine, brandy, and opiates as before.

February 16th, 9 a.m.—The patient was restless throughout the night, and this morning a good deal of fever present; respiration hurried; pulse 110, and hard; skin hot; a good deal of uneasiness referred to the chest, particularly beneath the clavicle on either side, together with a sense of tightness or constriction beneath the sternum. The wound free from pain, and discharge not arrested; pressed out an additional quantity on the hand being laid firmly towards the base of the posterior flap, and with the same power carried forwards. Ordered calomel, James' powder, and opium in small and repeated doses—the full opiates and stimulants still to be administered, with counter-irritation beneath the clavicles, and a blister over the sternum; also a diaphoretic mixture, with carbonate of ammonia, to be taken at intervals; the nourishment to be persevered in as before. 9 p.m.—The oppression beneath the sternum somewhat relieved; still the breathing remains considerably embarrassed; pulse still at 110, not so hard; skin cooler; no pain referred to the stump, and it looks healthy; to continue everything through the night as directed in the morning.

February 17th, 9 a.m.—The first glance at the patient told how critical was her position; the whole countenance was changed, sunken, and livid; hurried rapid respirations, imperfect; utter prostration, and almost inability to speak audibly; a staring vacant look, and an irritability on being roused to take either nourishment or medicine; with this fatal blood-poisoning, which the condition of the chest and accompanying symptoms too surely indicated, it was most remarkable how little aberration from a healthy state was yet manifested in the wounded part. Five hours had elapsed—the powers of life flagging lower and lower, though sustained as much as possible by the repeated exhibition of stimulants—and now only did the stump show any indication of the shock, the constitutional poisoning. To the educated eye the changes were now being characteristically developed; its full proportions became collapsed and even flaccid; the discharge, which up to this time was healthy pus and in full quantities, now changed its character to a more fluid, discoloured, and even reddish appearance, and materially altered both in consistence and quantity. 9 p.m.—All the poisoning influence of the introduction of pus into the system more clearly developed; pulse weaker—a mere thready beat; yet it is most remarkable the force and volume of voice with which the patient will suddenly scream out—I might almost say with an unearthly shriek. Eyes look vacant, staring, almost without motion of the lids; now

the capacity for swallowing is difficult, but upon a persevering trial fluids can be got into the stomach. Ordered that there should be no relaxation of efforts to convey nutriment and stimulants into the stomach.

February 18th, 9 a.m.—I was informed the patient died at four o'clock this morning; and even up to an hour before her death had swallowed some brandy and water. A careful *post mortem* examination was made of the body. First as to the condition of the stump: it was most remarkable the progress which was made towards repair; three-fourths of the extensive flaps were adherent, perfectly united together, and the outer edges of the wound were in a healthy granulating state as far as could be seen without violence to the parts; from this position, deep too, pus could be pressed out; but neither in quantity to excite apprehension, and but little changed from the marked healthy characteristics of the fluid; on forcibly separating the flaps—and to effect this it required more traction than I expected—the deep structures seemed quite healthy; all the surface of the acetabulum and the tissues surrounding it were coated over with organized lymph and incipient granulating surfaces—in short, nothing could present a more satisfactory appearance, tending to permanent repair.

On examining the ligatured main artery of the limb, a proceeding which I made with the most careful dissection, I found a demonstrative evidence of the facts so ably and clearly inculcated by Jones years ago—the division of the internal and middle coat of the artery—lymph now occupying the site of their division—lymph above and below the ligature, as it rested in its constricted bed of the external coat of the vessel; there was also a solid, well-formed conical coagulum filling up the femoral trunk to within the eighth of an inch from where the profunda artery passed off; this coagulum was absolutely adherent at its base, and fused into the lymph which had welled up from the internal divided coats of the vessel; it was likewise more firm and fibrous here than towards its upper part, which was softer in its consistence and conical in its arrangement. I found, on the most careful examination, the base or lower part of the column of the plug to be intimately incorporated and adherent to the lining membrane of the vessel for a little more than a quarter of an inch. From the most careful consideration of the condition of the parts I would most assuredly affirm that there was no risk of secondary hemorrhage—in fact the plug was very firmly adherent, and almost tissue in the line, where it was fused into the lymph exuded

from the wound in the internal and middle coats; and it is but reasonable to infer this sealing up of the calibre of the vessel would have been perfected long before the separation of the ligature; beside the tied end of the femoral artery hung collapsed the end of the femoral vein; it had not retracted; and though lymph was abundantly shed around these parts, and had to be torn through to expose them, yet the vein seemed, as it were, excluded from this vitalizing influence and effort at repair; no, the vein "hung collapsed," its edges softened, its coats bathed in purulent matter; on slitting up the vein its lining membrane was inflamed and coated over with pus; on proceeding about two inches upwards its calibre was distended with pus; it was also marked throughout the track of the external and common iliac vessels { pus was discovered, by the microscope, in blood taken from the cava, and pus was discovered in blood taken from the heart. } The abdominal viscera were next carefully examined, but no aberration from a healthy state could be discovered—no increased secretion from the lining membranes, or trace of inflammatory action. The chest was next opened; the heart was healthy in its muscular structure and fatty arrangement, and the pericardium showed no evidence of change; more fluid than natural was contained within it—somewhat more than a drachm. The examination of the lungs revealed numerous small deposits of purulent matter; these taken in conjunction with the condition of the femoral and iliac veins, loaded with pus—with the fact of pus discovered in the blood of the cava and in the blood of the heart, can leave no doubt as to the blood-poisoning of the person; this case affords a most remarkable example—the most rapid, in my experience, of pyemia producing its fatal effects in an incredibly short period of time. The rapid improvement of the patient after the operation, the freedom from pain, the cheerfulness and the buoyancy, and the expectancy of recovery—these all combined and appeared to render more certain, day after day, a fortunate issue, and as it were showed that no constitutional symptoms embarrassed recovery; and this constitutional improvement harmonized with the condition of the stump—the extensive wound, which presented throughout its course the most rapid amendment; and yet in a few hours the case is irretrievably lost by a complication that no human watchfulness, skill, or foresight could guard against. The examination of the limb shortly after its removal from the body fully revealed the true nature of the morbid growth as diagnosed during life. The bones implicated were the tibia and lower

extremity of the femur; they suffered to about the same extent from the malignant action—clearly from the history of the case, having its commencement in the former and spreading to the latter, with the implication of all the structures intervening and around; the head of the tibia was largely expanded, the inner and anterior wall of the bone been all eaten away, and filled by a large fungous mass, a deep and fixed portion of the general growth. The femur suffered likewise in the same way—all its osseous material entirely disintegrated and removed—except a remnant of the external condyle; a kind of elastic cartilaginous structure replaced it to a great extent; this structure, too, being peculiarly modified in certain places; in some true encephaloid cancer being deposited—soft, elastic, brain-like; in others sharp spiculæ of bone radiating from certain points, and diffused throughout the growth in greater or less masses. The lower part of the thigh and in the upper part of the calf it was impossible to recognize the muscular tissue; all had either been absorbed by the pressure from within, or more probably seized upon by the morbid action, and converted into malignant structure; the tumour, where most prominent and in large masses, was firm in structure, and presented in many parts a lobulated arrangement varying in vascularity; in some parts brilliant with red vessels, in others of a dark modena colour, while here and there scattered throughout were some soft points, even fluctuating, caused by broken up vessels; and in others masses of hard spiculæ of bone structure; the deeper portions of the growth, and all in proximity with the bones, presented the greatest density and immobility, while the more superficial parts were softer, more elastic, and channelled for the passage of larger venous trunks.

Two of the greatest authorities in Surgery have accurately described this form of tumour—the one in France, the other in England—each by a different name.

M. Boyer illustrates his views by a remarkable case, almost similar to mine, in his great work (*Traite des Maladies Chirurgicales*, Tom. III., chap. XXI., Art., IV.), and observes:—"Nous réserverons la dénomination d'ostéo-sarcome pour la dégénération du tissu osseux qui paroît se rapprocher le plus de l'altération et de la marche propres au cancer. Tout ce que nous avons dit précédemment sur le traitement du cancer, s'applique sans restriction à celui de l'ostéo-sarcome: anisi l'art ne connoît aucun moyen d'arrêter les progrès de l'ostéo-sarcome une fois qu'il est déclaré, et l'on ne peut opposer à cette cruelle maladie que l'amputation du membre, ou

bien un traitement palliatif." Sir Astley Cooper describes the disease in these words :—"By the fungous (exostosis) is to be understood a tumour of softer structure than cartilage, yet firmer than fungous in other parts of the body, containing spiculæ of bone; malignant in its nature, depending on a peculiar state of constitution and action of vessels; a disease somewhat similar to that which Mr. Hey has denominated fungous hematoides, but somewhat modified by the structure of the part in which it originates."—*Cooper and Travers' Surgical Observations*, Vol. I.

The formidable nature and dangerous tendency of the malady under consideration is borne ample testimony to by those celebrated men whose views I have quoted; and the case which I have detailed myself is perhaps as remarkable as any on record, for the rapidity of its growth and in assuming such huge proportions in a short time. Looking from the first upon the characters, the beginning, and growth of this disease as malignant, I applied the same rule of interference, the same law that should direct the surgeon as to operative measures when the same type of disease presents itself in other parts, namely—its entire removal, its severance from the body, through healthy, sound, normal structures. In malignant disease attacking the bones of the extremities the surgeon should always if possible, amputate *at* or *above* the next joint. I have said "at" if the condition of the soft parts will allow him; if not, then "above," away from the diseased contaminated parts, both soft and solid; under any circumstances I do not think it prudent to leave any portion of the affected bone behind; and though its lowest extremity be the part engaged I would urge the propriety of its removal *from* the joint above were even the integuments surrounding the bone above apparently ever so healthy. I have no doubt, from observation, experience, and even microscopical examination—the latter testified too by able hands, from specimens afforded by me for examination—that malignant disease once established in the end of a bone creeps through the bone structure, seizes upon and contaminates the medullary lining membrane, far away from the original seat of the mischief, long before the integuments show any indication of the pollution beneath; therefore, I repeat, the entire bone should be removed; I have laid down this point as an aphorism, and were it necessary I could adduce several instances, both in hospital and private practice, where such caution has acted efficiently, preservatively, in saving the patient for years from any return of the disease. From many facts accumulated I had no

hesitation in arriving at the conclusion of the propriety of the removal of the limb from the socket in this case; and though the patient did not survive, yet ample proof has borne testimony to the propriety of the measure; the release from suffering, the improved condition of the patient for several days—the increased strength, the inspired confidence of the sufferer, all point to the benefit obtained; again, and speaking far more forcibly in favour of its application, were the changes wrought in the wound, the greater part united by first intention, all the deeper parts agglutinated with adhesive organised lymph—pus from the granulating surface, healthy, and with ready access to the surface—the main artery of the limb sealed up, permanently and for ever—showing altogether a combination of circumstances and facts the most remarkable that could be adduced towards the salvation of the patient. The benefit derived from chloroform in this serious operation cannot be overlooked; all shock was removed both from apprehension of suffering, ineffectual struggles, and absolute pain; the patient was quite unconscious that the operation had been performed, and had no suffering at any time. A question has arisen as to the propriety of securing the main artery of the limb in the first instance; it is not at all necessary if the precautions such as I have laid down are carefully followed out; neither do I think the pressure on the abdominal aorta, no matter how effected, by tourniquet or otherwise, is at all to be depended on with the same trust as pressure at the groin, on the main artery of the limb, and the judicious arrangement of assistants to grasp the flaps, in front and behind, as the knife pursues its course; this practice is strictly enforced, as I would wish to inculcate from the case that I have detailed. I freely admit the horseshoe-shaped clamp or Professor Lister's tourniquet upon the abdominal aorta—and another, if considered necessary, even on the iliac vessel, may be advisable when trustworthy attendants cannot be procured—and where, in spite of all obstacles, a bold and dexterous surgeon is determined to give his patient the last advantage from his art; but I deny altogether the superiority of the mechanical contrivance to effect the object in view as contrasted with the educated hands of men trained in operative surgery. However, Professor Lister's clamp has acted most favourably in many cases, and I must not omit pointing out the high praise Mr. Erichsen bestows upon it (*Lancet*, Vol. I., 1866, p. 223); and I am quite convinced the instrument should be in the possession of every operating surgeon. Now there is one point to

which I have not referred in the division of the vessels, and the troublesome consequences that may follow if not attended to; I mean where hemorrhage is persistent from the femoral vein. In amputations of the thigh, high up, I have often been troubled with it; and when neither plug or adjustment of flaps would check it, have invariably pursued the practice, in the last few years, of passing a wire suture, by means of a tenaculum, with an eye near its point, through the skin, beneath and around the vein, and through the integuments in the opposite side of the vessel to where the instrument entered; thus the vein was pressed up by the ligature against the integuments, and a few twists of the wire perfectly controlled the flow of blood; the ligature was unravelled and withdrawn in twenty-four hours; and in no instances was there a return of this untoward complication. Should such a result take place after amputation at the-hip joint I would strongly advise the adoption of this simple method, which I have found so effective in other cases, much in the same class, rather than put a ligature on the vein. Throughout the whole history of this case it will be seen there was progressive improvement for several days; the shock was avoided by the exhibition of chloroform—the hemorrhage guarded efficiently against; scarcely an ounce of blood was lost; quickly and rapidly all vessels were safely secured; the stump was dressed with ease, and without pain in some hours after being left to glaze; the flaps were adjusted, bound together with wire sutures, and retained more permanently and throughout in contact, with adhesive straps and compresses; days past by; the patient regaining strength and cheerfulness, with all hopes of recovery, when suddenly pyemia becomes developed, a complication that never could have been anticipated from the favourable course of the case, yet, surely and steadily, and without compromise, it pursued its unrelenting course, poisoning the springs of life; but fortunately for our science and our art, the changes produced upon the wound are conclusive as to the reparative efforts which may be looked upon and expected with certainty, even under such trying circumstances, in so fearful an operation. From what has been stated it will be seen that the soft parts were all nearly united together; that the main artery of the limb was sealed up and blocked, beyond any danger of secondary bleeding; that the discharge was very small, and the entire granulating surface was healthy; in fact all the dangers of this great operation were overcome, when suddenly a complication that may arise from the simplest wound presented itself—the absorption

of pus into the system; and so this woman perishes. This disheartening sequence to an operation, correctly conceived, judiciously carried out, and affording such demonstrative proof, from the *post mortem* examination, of the salutary changes brought about in the divided parts, can never militate against its advantages or lessen the ardour of true surgery, but must rather impress the mind of every thinking observer, of every practical man, with the powers of operative surgery to save, when all other powers are abortive in rescuing the sufferer from pain and death.

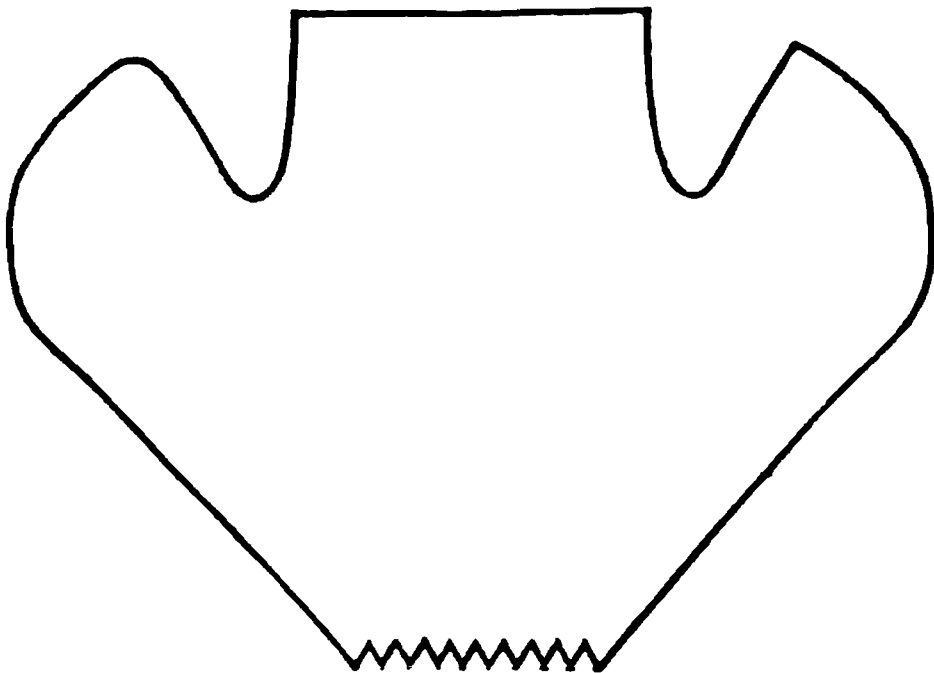
ART. VIII.—*Contributions to Operative Surgery. Part I.*—

Operations about the Face. By MAURICE HENRY COLLIS, M.B. Univ. Dub.; F.R.C.S.I.; Surgeon to the Meath Hospital and County Dublin Infirmary; Sometime Examiner in Surgery to the Queen's University in Ireland, and Member of Council of the Royal College of Surgeons in Ireland, &c.

1. *Lupoid Destruction of Nasal Cartilages, with Occlusion of the Remainder of the Nostrils, and great Deformity; Rhinoplasty.*—Peter Leahan, aged twenty, was admitted into the Meath Hospital early in March, 1865. He had suffered for many years from lupoid ulceration of the nose, and to a slight extent also of the cheeks. By a varied line of treatment I checked the spread of the disease, and ultimately got the ulcerated parts to cicatrize. Lupus requires much patience, as all surgeons know, and I fear we too often do not pay it a sufficiently close attention; we weary of it, and the patient perceives this, and wearies of us. It is certainly a most tiresome ailment; but yet, by patient watching of its varying phases, and adaptations of remedies to suit each change in its condition, we shall generally succeed in curing it in the long run. In the present case I used iodine locally during the cold weather, and changed my hand for soothing applications according as the harsh winds of Spring subsided. I finally healed the ulceration with a lotion or paint composed of a drachm of watery extract of opium to an ounce of water. This was applied frequently through the day; by the beginning of May there was complete cicatrization of the ulcer and entire absence of new pimples or tubercles in the surrounding skin. Accordingly on the 12th of May I set about repairing the deformity of the nose. I first cut a piece of adhesive plaster into the form of flap, which I purposed raising

from the forehead. The outline of this flap is given in Fig. 1.

Fig. 1.



I measured this flap accurately, so as to adapt it afterwards to supply the deficiency of the nose, allowing for the subsequent shrinking of the material by absorption of the subcutaneous adipose tissue. I now pared the edges of the nose, or rather of its stump. I found but little of the cartilages left, except in the median line, where a very fair amount of cartilage still remained, compared with what was left of the alæ. I now placed the piece of adhesive plaster upon the forehead, the expanded and deeply-notched margin being laid as close to the hair as possible, so as to leave a long pedicle. This mode of outlining the flap has an advantage over ink or other materials, inasmuch as it does no harm to the flap or its margins, nor does it stain the cicatrix as ink will do. I now cut out my flap, removing only the integument and subcutaneous fatty tissue, and such muscular and aponeurotic fibres as were intimately connected with the skin. I did not require any new bone in the nose, as the nasal bones were not implicated, and consequently Langenbeck's plan of raising the periosteum in the flap was not needed. The flap was now twisted round and applied to the raw surface prepared for its reception. This part of the arrangement is facilitated, and the healthy circulation of the flap ensured, by prolonging the incision on one side down towards the angle of the eye, along the wrinkle or groove which is made in that spot during the action of frowning. I always make this cut towards the patient's left eye, turning the flap round by the right; this is simply more convenient to my hand, but either side will do. The flap was now fixed in its place by a number of fine sutures placed from two to three lines apart. On one side I used fine silk, on the other silver wire. I did this to test their relative value. The

result was that I had to remove the silk threads in forty-eight hours from rapid ulceration, while the wire remained in for a week or longer without producing anything beyond the most trivial suppuration; but in the end there was no mark along the side where silk was used, while the cicatrix on the side where wire was used was marked, and remained red and elevated so long as the patient remained under observation. This point is deserving of note.

It will be perceived that the centre of the flap (the portion which was to complete the columna) was made much broader than usual. This is an improvement for the knowledge of which I am indebted to my young colleague, Mr. Stokes, whose interesting paper on Rhinoplasty will be found in the pages of this Journal for May, 1865. The object of this breadth is to enable us to form a substantial columna at the same time as the rest of the nose. All went on well in this case; the flap adhered throughout, and except that there was a tendency in the nostrils to contract, the new organ could be said to be almost perfect; however, an occasional plug of seaweed kept the nostrils patulous. On the 14th June I divided the pedicle, and in a few weeks after he left the hospital exceedingly proud of the improvement in his personal appearance. In Plate I., Figs. 1 and 2, there is a faithful delineation of the appearances in this case before and after the operation. The drawings are from photographs by Forster, of Westmoreland-street, who has on many occasions assisted me signally in preserving records of my cases in this necessarily accurate manner. Copies of these photographs are deposited in the Meath Hospital and in the library of the College of Surgeons.

2. *Epithelioma of the Nose; Third Stage, Frightful Deformity; Removal of Diseased Portion, and Indian Rhinoplasty.*—Mrs. Shea, aged seventy-five, was admitted into the Meath Hospital early in March, 1866, for extensive epithelioma, which had caused destruction of the alæ of the nose, and great deformity (Plate II., Fig. 1). She stated that nine months before her admission into hospital a small wart upon the side of her nose began, after years of quiescence, to annoy her. She picked it, and it rapidly grew larger. In a wonderfully short time it had destroyed much of the skin, and had commenced to spread under the margins of the ulcer thus formed. The drawing gives a good idea of its condition when I first saw her. There were two large ulcers running into one another, with the usual

PLATE 1

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Fig 1

Fig 2

MR MAURICE COLLIS'S CONTRIBUTIONS TO OPERATIVE SURGERY

BY A. A. COLEMAN

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MR MAURICE COLLIS'S CONTRIBUTIONS TO OPERATIVE SURGERY

Edm Bros & Co Ltd Dublin

Fig 1

warty floor and irregular hypertrophied margins. The amount of hypertrophy was very remarkable. Fortunately the ulcerative process had spared the very tip of the nose, so as to leave an appreciable amount of the columna sound, and a small piece of each ala nasi. The disease was as yet local; the glands about the parotid region and the neck were free from contamination. She suffered much pain and occasional hemorrhage to a slight extent; the purulent discharge, as usual, was foul and profuse. The spread of the disease was rapid; the woman's general health was excellent, and in spite of her advanced age I gave her the chance of a prolonged life and freedom from much misery which an operation afforded. She was most urgent for the removal of the disease, independent of any idea on her part that I could restore the organ to a tolerably respectable condition, so that when I promised to give her a new nose I had no peace until I redeemed the promise. March 14, having chloroformed her, I removed the entire mass, sparing the extreme tip of the nose and the free margin of each ala. The infiltration of epithelial tissue had extended deeply into the cartilages, so that with the exception above stated all had to go. Upwards my incision extended to the middle of the nasal bones. I had thus a triangular gap one inch and a quarter in length to fill up by a flap taken from the forehead. Owing to her forehead being very low I had some difficulty in getting enough skin free from hair; however, by taking it a little obliquely I managed to get what did very well; and twisting the flap round I fastened it in its new berth by horsehair sutures inserted very close together—not more than three lines apart, and in some places as close as two lines. The flap, which became very blue and cold, was covered with the softest cotton; wine and opium were administered, and a little hot punch. I should mention that in dissecting up the flap the greatest care was taken not to handle it needlessly. Had it experienced any rough handling I have no doubt it would have sloughed. As it was, it recovered its colour and warmth in a few hours, and in the end united throughout without the smallest sloughing. The sutures were removed according as they seemed to have done their work—some in a week and others not for a fortnight. The pedicle was divided on the 13th April, and soon afterwards the patient left hospital with a remarkably fine nose. The wound in the forehead healed rapidly, and had almost closed by the time the photograph was taken, five weeks after the operation. See Plate II., Fig. 2.

It may be objected that such an operation was uncalled for at the patient's advanced age, and that from the rapid growth of the disease relapse was more than probable; also that at her age the risk of failure was great, owing to the low vitality of the skin. Of these objections the last is the only one that seriously touches the question. As regards the patient's age I found that she considered it no obstacle; and indeed from her hale appearance and the rapidity of the healing process, I think she is as likely as not to live to ninety. Certainly she was as anxious to be rid of her disease as any girl of nineteen. Looking on epithelioma of the skin as a perfectly local disease at the commencement, and only in a very minor degree liable to relapse if freely and perfectly removed, and seeing that there were no glandular enlargements, I deemed it improbable that we should have any relapse. I have often removed epithelial growths even at advanced ages, and I have seen so few cases of relapse while the disease was confined to the skin that I have not the dread of relapse which some who have written upon the subject appear to have. In the present case relapse could hardly make matters worse than they were, and to refuse to operate lest a relapse should occur would have seemed weak and cowardly. I do not know if there be a difference between the epithelioma of England and that of Ireland. Certain it is, that we seldom have relapses from epithelioma of cutaneous or border tissues in this country. I have operated on a man of seventy-two, who is now in his eighty-sixth year, and on many others nearly as old, who remain free from relapse for periods varying from five to ten years, so that age or fear of relapse would not prevent my operating if general health was good. As regards the risk of the flap sloughing, no doubt in the aged this accident is more likely to happen, but this only makes it more incumbent on us to handle the flap as gently and as little as possible during the operation. By supplying stimulants freely and by covering the parts well with soft cotton the heat of the flap is soon restored; nor should such a flap be too early disturbed by attempts to dress it, or by too curious inspection. The result of this case proves that plastic operations of much delicacy will succeed in the aged if due care be taken.

3. *Destruction of part of Upper Lip and Cheek by Cancrum Oris; Great Deformity; Plastic Operation.*—Owen Graham, aged six years, a fine intelligent child, was admitted into the Meath Hospital in June, 1865. When an infant he had suffered severely from cancrum

PLATE 3

Fig 1

Fig 2

oris. The entire thickness of the cheek had been destroyed from the angle of the mouth backwards beyond the molar teeth, and upwards, so as to expose the cavity of the mouth and the gums and teeth. Even when the mouth was closed the deformity was very great. On the 23rd of June I vivified the edges of the gap, detached its margins from the bones so as to give the boy greater power of moving his jaw. Dissecting a long triangular flap from the side of the chin, I turned it up so as to complete the upper lip and close the gap in the cheek. Numerous points of suture held the flap in its place; and as the pedicle contained the main trunk of the superior coronary artery it was well nourished, and it rapidly united. The deformity was very much diminished, and as the boy grows up it will scarcely be remarked. The space from which flap was taken was covered over by drawing the edges together, and owing to the flexibility of the young skin there was no difficulty in bringing about primary union. Similarly the flap, which was of a long triangular shape, accommodated itself to the irregular outline of the gap, and became capable of filling a space much broader and somewhat shorter than the flap had originally been. The drawings give an excellent idea of the appearances before and after operation (Plate III.).

4. *Epithelioma of the Angle of Mouth and of the Cheek—Third Stage; Extensive Destruction; Removal of Diseased Mass; Recovery, and Freedom from Relapse to the Present Date (Eleven Years).—*—Byrne, a baker and gardener, from the town of Arklow, aged about forty-five, was sent to me in the year 1855, by Mr. L'Estrange. He had an extensive ulceration of the left cheek and angle of the mouth; the disease was plainly epithelial, and had gone on to the third stage, namely, ulceration and infiltration. One fourth of the upper and nearly as much of the lower lip was destroyed, and a square aperture in the cheek allowed the teeth to be seen. The edges of the ulcer were thickened and warty, prone to bleed on the least touch, and pouring out a copious and very fetid discharge. The skin all round was infiltrated to the distance of two or three lines; yet the glands were free from enlargement. The disease had commenced in the usual manner, by a fissure with hardened edges at the angle of the mouth; it had taken several months to reach its development at the date of the man's admission into hospital. I removed it early in November, 1855, by an incision which formed three sides of a square. The cut edges when brought

together by hare-lip pins formed a kind of leech-bite wound. They healed rapidly and without any ulceration. The cicatrix, at first firm and thick, became gradually fine, and the cheek and lips, at first somewhat puckered and restricted in mobile power, became in course of years relaxed and capable of almost as much motion and distensibility as before the disease attacked him. He came to see me from time to time, dreading a relapse; it is only within the last two or three years that his mind has been at rest upon this point. In this case I used no special treatment after operation to prevent relapse; he continued to follow his occupations as before, and has been free from illness of any kind up to the present date. The second portrait, taken a few days ago, shows the absence of deformity and the freedom from relapse which signally characterizes this case (Plate IV., Fig. 1). For the great accuracy of these lithographs I am much indebted to Mr. Lewis.

5. *Epithelioma of Angle of the Mouth and Inside of Cheek; Removal of a portion one inch and a quarter square; Appearance of the Disease in a portion of the Lip distinct from that first attacked; Removal of Surface of Lip.*—John Neal, aged sixty-six, also from Arklow, a fisherman by occupation, has had epithelioma of the angle of the mouth for some months; this has gradually extended so as to destroy a large portion of the cheek and to leave a space through which the teeth can be seen. The amount of destruction is about the same as in Byrne's case. April 17, 1863, I removed a mass of disease by three incisions, forming three sides of a square one inch and a quarter along each side, the fourth side being represented by the irregular margin and warty masses of the diseased structure at or about the angle of the mouth. The margins were brought together by several pins, and by interrupted sutures in the intervals. All healed well and rapidly, and the man returned to his work. In October, 1864, he returned to hospital, as epithelioma had appeared along the margin of the lip. I found the new growth scattered along the surface of the margin of the lip, not extending deeply and not in a continuous line, but here and there in fissures or cracks along the surface. There was a perceptible space of sound skin between the old cicatrix and the nearest part of the new growth; and the old cicatrix was thin and healthy, and there was no appearance of infiltration in its neighbourhood. I looked upon the disease in the lip as a new outbreak and not as a relapse, and I counselled its removal. I

PLATE 4

Fig 2

Fig 1

M^r MAURINE COLLIS' CON. RHINOTOMY TO CLEFT-LIP SURGERY

accordingly shaved off the margin of the lip in the usual manner and so relieved him from the pressing danger of extensive ulceration of the lip and infiltration of morbid matter into the chin and submental glands. The parts healed kindly, and all has gone on well since.

6. *Fibroplastic Ulcer of the Angle of Mouth and Cheek; Removal of Diseased Mass.*—James Igoe, aged thirty-five, from the neighbourhood of Kilcock, came to me in May, 1863, for advice relative to a very peculiar form of ulcer at the angle of the mouth. It had not the ordinary warty appearance of epithelioma, nor was it a syphilitic ulceration. The margins were ragged and overhanging, but not capable of sub-division into papillæ, as the epithelial growths are. The floor of the ulcer was even; the secretion not very copious, not peculiarly offensive, and the pain was excessive.

The disease had originated in a fissure at the angle of the mouth, which had extended more especially on the mucous surface of the lips and cheek. The skin was affected, but not as extensively. The ulcer was not epithelial, nor was it syphilitic, nor did it resemble any form of lupus I had ever seen. I removed it freely with a knife, on the 20th May, 1863, by a four-sided incision. Inspection of the growth under the microscope showed it to be a fibroplastic ulcer. The subsequent progress of the case was satisfactory; the wound healed, and the disease did not reappear. Various caustics had been applied without effect before I had recourse to the knife.

7. *Fibroplastic Growth from the Periosteum of the Nasal Cavity; Removal by a New Operation of Great Simplicity.*—Margaret Biron, aged fifty, was brought to me in January, 1864. She had some obstruction of the nasal passages from childhood, and had consulted the late Mr. Carmichael and other surgeons. They appear to have recognized her disease as something more than ordinary polypus, and advised her to let it alone. Gradually the tumour enlarged, expanding the bones of the nose, especially on the left side, and completely blocking up the cavity of the left nostril; the vomer also was pushed to the opposite side. Of late she suffered from headaches, sometimes also from severe pain about the region of the frontal sinus; her memory became weak, and indeed her whole mental powers were below par. Breathing was impossible through either nostril. Numerous dilatations of the angular and other veins showed themselves about the root of the nose, the eyebrows, and neighbouring parts of the cheeks; all these evidenced a state of

considerable pressure. On inspecting the tumour through the nostril, it showed itself quite close to the aperture of the left nostril; its surface was smooth and polished, of a red colour, somewhat brighter than the ordinary mucous polypus: a probe could be passed up on the inner and under sides with tolerable ease. All round the outer and upper surfaces of the tumour the probe was stopped at distances varying from an inch to a couple of lines. As far as one could feel, this was caused by the attachment of the growth to the walls of the nostril in these situations. I resolved to remove as much of the growth as I could include in a wire snare. It was evident that a disease which had lasted for at least thirty years could not be very malignant, and I considered it possible to obtain by this measure at least temporary relief, as well as an opportunity of ascertaining the minute structure of the growth. Accordingly, on the 6th January, 1864, I removed a portion about the size of a walnut, by the snare; and further, by means of the forceps, compressed those parts of the growth which I could seize; smart hemorrhage resulted from this rough handling, and on its cessation much relief was experienced from the frontal pain and headache; and in a few days a considerable amount of breathing power was restored through the nostrils. The piece removed was hollow, like a shell, covered on its external surface with thin mucous membrane, and lined with a curious mammillated growth; its basis was made up of an expansion of the lower spongy bone. The shell of bone was thin, hard and brittle, and under the microscope presented the ordinary appearance of bone. The mucous membrane covering it was perfectly healthy. The internal growth was of the fibroplastic or myeloid type. I have frequently seen this form of growth in connexion with tumour of the jaws; this is remarked by Mr. Paget. The patient got so much relief by this partial operation that she went home; and it was not until January, 1865, that I again saw her. The tumour had grown again so as to fill the entire nasal cavity as before, and the pressure upwards was greater. Her mental condition was lower than before, and appeared to those about her almost hopeless. On the 25th January I proceeded to operate after a new method, by which I hoped to remove all the diseased structure without destroying any of the bone that was healthy. Such a tumour would generally seem to call for complete removal of the superior maxillary bone; whereas by the method about to be described the disease was easily removed without serious injury to the sound

bone. An incision was made along the side of the nose where it joins the cheek; the incision reached from a point just below the inner canthus to the nostril. On the one side the cartilage of the nose was severed from its attachment to the superior maxilla, and the skin of the nose was dissected up along with the cartilage, so as to leave the nasal process of the maxillary bone and the greater part of the nasal bone exposed almost to the mesial line of the nose. On the outer side a flap of skin was lifted so as to lay bare the bone as far out as a line drawn from the centre of the lower margin of the orbit to the most external part of the nasal aperture. By means of a strong bone scissors the bones were cut in the lines referred to, namely, from a little external to the raphe or junction of the nasal bones below, across the nasal bone and the nasal process of the superior maxilla to the union of the latter with the frontal bone; and, secondly, in a line from the outer margin of

the bony nostril to the centre of the orbit. Fig. 2 represents alike the deformity which this patient suffered under, and the lines of incision above referred to. The black line beside the nose is the line of incision of the soft parts; the two dotted lines represent the extent to which the cutaneous flaps were reflected and the lines of incision through the bones. The flap of bone loosened by these incisions was irregularly four-sided, about an inch wide below and one and a quarter above. It was simply turned up, lifted on its hinge, like the lid of a snuff-box, and held so, without impairing its attachments above at the margin of the orbit. This manœuvre enabled us to see the left nasal cavity and its contents. The tumour seemed to have extensive attachments to the walls of the nose; the periosteum of the superior maxillary and spongy bones and of the vomer were continuous with its covering. These were freely divided with the knife, and then, partly by cutting and partly

by tearing, the morbid growth was removed piecemeal. It was very friable in texture, unless where it had a basis of bone, so that we were not able to form any idea of its shape from its fragmentary remains. Having cleaned out the cavity, and stuffed it lightly with pledgets of lint with strings attached to them, I closed the wound by restoring the bony lid to its place, and by bringing the flaps of skin across it, and uniting them with numerous points of suture. Considerable consecutive hemorrhage arose in the course of the day, which was checked by the local use of perchloride of iron in solution. She subsequently had a very severe attack of erysipelas, but recovered, and I saw her in excellent health and restored mental power in September, 1866, having had no return of the disease so far.

The operation is an eminently satisfactory one; it is simple and effective, and leaves little mark; this poor woman is scarcely marked—the line of cicatrix is no more than barely perceptible (Plate IV., Fig. 2). Above all, the operation is eminently and truly conservative, as it prevents the necessity for extensive removal of sound bone. The flap of bone united firmly and without deformity; this was mainly owing to my having left plenty of soft tissues in connexion with it when raising the skin from its surface. Another reason for keeping the knife closer to the skin than to the bone was the desire to leave unimpaired such branches of nerve as lie near the bone in this situation.

8. *Exostosis of Vomer; Removal by the New Operation; Immediate Relief from the Sufferings of Years.*—Jane Owens, aged fifty-three years, residing in No. 10, Fitzwilliam-lane. This woman got a blow on her nose eight or nine years ago. Four years afterwards she noticed a white track on the right side of the nose, just at the junction of the cartilage and nasal bone. Three years ago a piece of dead bone came out of her mouth, and other bits she picked out of her nostrils. Besides this, she got a kick of a cow on the left malar bone, which caused extensive abscess, loss of bone, and lachrymal fistula on the left side. These latter mischiefs, however, have nothing to say to the state of disease which we have now to relate.

She came to me about the 1st of July, 1866. Her nose was greatly increased in width, and probably also in height of bridge; the cartilaginous parts of the nose were not altered in form (Fig. 3). On inspection of the nostrils a white shining tumour appeared almost

Fig. 3.

at the orifice of the right nostril. A similar appearance was seen in the left, but not prominent. There was a copious purulent discharge from both nostrils, and also down the throat from the posterior nares; this pus was most offensive to her as well as to those about her. She is a person of most cleanly habits, and did her best to keep down the fetor, but she said it constantly made her sick. The sense of smell was not diminished. Touching the tumour with a probe it felt hard, and rang like a stone. The probe could not be passed along the inner side at all upon the right side, and only a short way along the outer, catching against projections of the growth; on the left the probe passed rather more freely; touching it in this way gave no pain; it was not movable. Of its nature and connexions I could form but an imperfect idea. It was connected with the septum on the right side, and probably also on the left. It was some form of exostosis, but from which bone it had originally sprung was not clear. On looking into the mouth no change in the bony vault could be made out. The chief alteration in the bones was at the root of the nose, where the space between the eyes was much increased, and downwards along the bony prominence made by the nasal bones. All things considered, I felt disposed to look upon it as some irregular form of necrosis or exostosis of the vomer.

As the amount of hypertrophy was greater on the right side, I determined to attack the growth on that side, and to be guided by circumstances as to the extension of the operation on the left. On the 4th July I performed the operation described in the preceding case—namely, the incision of the soft parts from nearly

the angle of the eye to the nostril, cutting off the cartilage from its attachment to the superior maxilla, and turning it up along with the skin on both sides of the cut off the right nasal bone, and the nasal process of the right maxillary bone, and off the side wall of the nose (facial surface of maxillary bone) outwards as far as a line drawn through the centre of the orbit. The bones were then cut with the bone forceps so as to leave a flap of bone nearly square, free on its inferior side, loosened by the forceps on its inner and outer sides and still attached above. This flap of bone was turned up and held out of my way by one of my colleagues. I then seized the bony growth with Fergusson's lion forceps, and partly by force and partly by manipulation I got it out entire. There was a copious flow of pus, with shreds of lymph and dead periosteum in abundance, and but little bleeding. The size and shape of the tumour and the outline of the cavity from which it was extracted showed at a glance that the masses in each nostril were parts of one growth, and that we had got it all out. This was no small relief to all parties; for although the patient was partially chloroformed, she had begun to be unruly, and it was not easy to keep up the chloroform, as she was in a semi-erect posture. I plugged the cavity with lint, soaked in the muriated tincture of iron, and bringing down the flap of bone I stitched across it the two flaps of skin with horsehair sutures. All went on well. The lint was removed the next day. Injections of a five-grain solution of chloride of zinc kept the cavity clean. The sutures held nicely, and were removed from day to day, the last so late as the 16th July, a fortnight after the operation. She left hospital that day, exposed herself to cold, and got a large abscess in the tonsil, but without injury to the parts operated on. She had a slight ectropium of the lower lid, which necessitated a trifling operation for its removal.

Her appearance is as yet but slightly altered. There is some diminution in the width of the upper part of the nose, and no doubt in time the bones will return to somewhat of their proper form.

Fig. 4 represents (with Mr. Oldham's usual accuracy) the appearance of the growth in profile; the projecting spur, shown to the right of the figure, is the only part in which the natural structure of the bone appeared unaltered; by this point it was attached above. To the left and lower part of the figure are seen two masses which projected, respectively, into the right and left nostrils; the cartilage of the septum fitted into the space between these projections.

Fig. 4.

Fig. 5 represents the tumour as seen from below. The tumour was composed of shell-like masses of bone, very hard, and by no means brittle. Exostosis of the vomer must be a rare disease—I cannot remember ever to have read of a case at all resembling this.

Fig. 5.

9. *Necrosis of a Large Portion of Superior Maxillary Bone, Removal by Incisions in the Mouth.*—This case—one of many similar—exemplifies the mischief from exposure to cold after the loss of a tooth, with more than usual severity. The operation for removal of the bone was trivial, and is one that most surgeons have had to perform many times.

John Cunningham, about twenty years of age, living in the county Wicklow, had a large upper molar, which became carious. In extracting it much violence was done to the jaw with the key, and one of the fangs of the tooth was left behind. He walked some miles that day in wet and cold, and as a natural result he got inflammation of the jaw. Abscess followed; and on its breaking a large piece of bone was found to be loosened; three teeth fell out,

or were pulled out by himself as they became loose; and finally, after several weeks, he came to me suffering from constant irritation and discharge of matter. The removal of the dead bone was easily effected by a single incision in the mouth. It was a large piece of bone, and had carried four teeth; in shape it differed from those I had previously seen perish in a similar way, as it ran up to a point in the centre of the outer plate opposite the fang which had been left in. Generally the necrosis, when incomplete, is limited to the alveolar processes, and the piece which perishes is of an oblong outline. This was the only peculiarity of the case, and the only reason why I include it in this series.

10. *Epithelial Disease of the Tongue near the Root; Removal of the Disease; Respite for Two Years.*—Patrick Gallagher, aged fifty, by trade a baker. Three months before his application for admission into hospital he observed a sore spot on the left side of his tongue, far back. I found he had a gap in his teeth where he carried his pipe, and that the end of the pipe used to be steadied against the tongue at or near the point where the sore began. This sore ran the usual course, not extending much in superficial extent, but steadily becoming deeper, and as it deepened producing hardness of the tongue from epithelial deposit all round its margins and involutions. The usual round of caustics had been tried with the usual baneful effect. The disease had been aggravated, and in three months had come to implicate about a fourth of the tongue. It extended forwards rather more than half way, leaving about the anterior third of the organ quite free; it came quite to the mesial line, but not beyond it; below, it could just be isolated from the lower jaw and its periosteum, and, posteriorly, it went as close as possible to the soft palate and to the folds which connect the epiglottis with the upper surface of the tongue. It was manifest that in a very short time the epiglottis and larynx must be attacked, when death would inevitably ensue from oedema glottidis. I therefore determined to remove the disease if possible. On the 28th January, 1863, having partially chloroformed him, I made an incision through the cheek from a point a little below the angle of the mouth back to a point opposite the last molar tooth in the lower jaw. I did not commence the incision from the exact angle of the mouth, because such a cut always leaves an ugly pucker by interfering with the orbicular tendon and the insertion of the muscles at the angle of the mouth, while a cut made

above or below this point produces much less deformity. This opening in the cheek gave room to get a strong Liston's needle through the base of the tongue behind and under the disease. The double ligature with which this needle was armed enabled me to pull through the opening the chains of two ecraseurs by which the diseased mass was isolated before and behind, and slowly separated; the division of the inferior attachments was effected partially by the knife before the ecraseurs were tightened. The chains were kept from slipping by hooks and strong needles plunged into the diseased part until they had begun to cut it. Fully ten minutes was taken in working the ecraseurs home, when the piece came out easily, and with scarcely any hemorrhage. Some solid perchloride of iron was passed over the raw surface of the tongue, and the wound in the cheek was closed with pins and sutures. All healed quickly. The tongue swelled very little, and the cicatrix, which subsequently resulted, was firm and healthy. The tip of the tongue pointed rather towards the left, but the articulation was perfect, and the relief from pain and salivation was complete. The disease did not begin to reappear for a twelve-month, when it slowly showed itself in the gland under the ramus of the jaw; and upon inspection, in a slight degree far back near the under surface of the base of the tongue, in the cicatrix. I saw the patient alive in January, 1865, two years after the operation. He was then not likely to live many months, as the disease had extensively spread through the cervical glands; the pain was severe, and in all probability killed him soon after. There was no external ulceration.

11. *Epithelial Disease of the Tongue; Similarly treated.*—Darby Sullivan, aged sixty, from Listowell, in the county Kerry. This man had an epithelial ulcer on his tongue which implicated the greater part of the left half of the organ from near the root to the tip. It was by no means so favourable a case as the last, but he wished to have the chance of operation, and I accordingly performed a perfectly similar one to that which Gallagher underwent. The same incision was made in the cheek, and two ecraseurs were employed. Considerably more of the tongue was removed, but with all the care I could use I was not satisfied that all the diseased parts on the floor of the mouth were removed. All healed up well for the time, but in August, 1863, four months after the operation, I heard that the disease had returned in the neck, and

with severe suffering. He got rapidly worse, and died within the year 1863.

Comparison of these two cases proves, as far as they go, that the more completely the disease is removed the longer is the respite. I have in these pages and elsewhere so often given forth my views on the local nature of epithelioma that it is unnecessary to repeat them at any length. This disease is essentially different from cancer, in its origin being superficial and not interstitial, in its rate of progress being slower, in its longer limitation to the place of origin, and in its far greater curability. It is not cancer of the skin—which is a well-known and easily-recognized variety of tubercular scirrhus; nor has it a right to be called cancer any more than the fibroplastic, recurrent fibroid, or other lymph growths, which are more analogous to cancer in their mode of origin, and which are yet distinguishable from cancer, and distinguished from it by all scientific surgeons of the day. That epithelioma of the tongue is more virulent and rapid in its progress than the same disease on the lip is intelligible from the higher vascularity and moisture of the tongue: but we may surely accept this fact without asserting, as a most illogical consequence, that because it is here exceptionally rapid and destructive, it is therefore cancer. Yet to what else do the arguments in favour of its being cancer come.

12. *Epithelioma of the Right Side of the Tongue; Removal by Section of the Jaw and Knife; Death from Pyemia.*—John Young, aged sixty-five, had been suffering from epithelial disease of the tongue for about five months. The right half of the tongue was diseased from tip to base, and the floor of the mouth very much ulcerated also; the disease came quite up to the mesial line in the middle of the tongue, and nearly so at the tip. I found it would be impossible to take all away without section of the lower jaw, so on the 21st June, 1865, having chloroformed the man, I divided the lower jaw at the symphysis, by saw and bone forceps, having previously split the soft parts from the lip to the os hyoides in the mesial line. Having seized the tongue, and drawn it forwards, with a strong hook, I split it rapidly down the centre to the base, going as near the epiglottis as possible. Hitting the exact raphe the operation was very easy, the least touch of the knife splitting it fairly down the centre. Before detaching the diseased part I had a very strong ligature passed as far back as possible behind the diseased parts, and tied with the utmost tightness. I then carefully

cut across the base, and removed the mass without arterial hemorrhage. On the stump I picked out and tied a couple of arteries, which was a fortunate precaution, as the strangulating ligature loosened and slipped off very shortly. The parts were brought together, the teeth tied together with wire; sutures were put into the lip and chin, and an aperture was left below the chin through which the ligatures were brought, to serve as a drain for saliva, blood, or pus. The man had a rigor, but rallied under stimulants. However, a bad style of discharge always came from the wound, which looked grey and sloughy. In spite of antiseptic lotions, good diet, stimulants, and quinine, this continued; he became yellow and pinched, and finally died with symptoms of what is termed pyemia, on the 5th July, a fortnight after the operation.

There is no doubt the mouth is a locality most favourable to the direct absorption of foul secretions; to this I think we must attribute the great fatality of operations such as this.

13. *Epithelioma of the Tongue; Division of the Gustatory Nerve, with Unusual Benefit; Repetition of the Operation on the other Side, with Relief.*—Michael Kennedy, aged about fifty, had been for fourteen months the subject of a severe ulceration of the left side of the tongue. This was originally produced by the irritation of a ragged tooth—the first molar in the lower jaw. The ulcer extended from the apex of the tongue back to a line beyond the last molar tooth; it was also broad, occupying not only the side, but the under surface, and much of the dorsum of the organ. The teeth indented it deeply, especially the first molar. The sub-maxillary and lymphatic glands about the angle of the jaw were much enlarged; the ulceration extended slightly to the right of the mesial line, and deep infiltration had occurred in the body of the tongue, giving it a nodulated appearance and a hard feel, such as might have made it difficult at first to say whether the disease was scirrhus or epithelioma; but the early history clearly pointed to the latter affection. The pain was constant and intense, precluding proper sleep, and incapacitating the poor man for any work. This pain had been an early symptom, dating in fact from the very commencement of the ulceration, and arriving at a distressing intensity at the beginning of 1863. The man had been the round of the principal London Hospitals, and had suffered much from caustics of various kinds. Narcotics of all sorts had been used ineffectually, or with transient benefit. Strange to say no one had suggested the

division of the gustatory nerve as a palliative, although it was quite the case for it, the disease being still limited to the region supplied by this nerve. I proposed it to him, and he acceded at once. Indeed his sufferings were so acute that he would have borne anything for relief. There was little difficulty in the operation, which I performed on December 8, 1863, in the following manner:—placing him in a chair opposite a good light, I passed my left index finger into the mouth, until it rested on the last molar tooth. Then, introducing the point of a strong curved bistoury beyond my finger, I made an incision of less than an inch in length along a line running obliquely in the direction of the angle of the jaw. This incision lay about half an inch beyond the last molar tooth, and three-quarters of an inch below its level. The parts were divided down to the bone. By such a wound, the gustatory nerve and, perhaps, a few fibres of the mylo-hyoid muscle are divided, as they lie beneath the mucous membrane. The pain of the incision was considerable, but momentary. That night he slept well, for the first time for eleven months enjoying rest unbroken by pain. The ulcer next day was insensible when touched, and quite devoid of pain, and it so continued for six weeks. This was only the natural effect of division of the nerve; but in a few days an unexpected benefit arose to the parts from the absence of pain; the surface of the ulcer lost its vivid angry hue, while the edges became of a bright healthy red, and were evidently closing in. In the course of a week the ulcer was considerably reduced in size, and in three weeks the upper surface and side had healed, leaving only a small warty patch on the under surface raw, but not tender. I removed the obnoxious tooth about the end of the second week, as soon as it became evident that if left there it would press on the side of the tongue. That so large a portion of the ulcer should have healed in this singular manner affords a strong proof of the assistance which freedom from pain confers upon all actions of repair.

At the end of three weeks he began to feel pain on the right side of the tongue, and I divided the nerve on that side also, with great benefit to him. The operation, although only palliative, was of great benefit to the poor man, so long as the disease was limited to the anterior part of the tongue. From the amount of glandular infiltration it was plainly impossible to remove the disease by any surgical operation, and under those circumstances it was a great point to be able to afford even temporary relief in this simple manner.

The operation of dividing the gustatory nerve was first performed by Hilton, whose case is given in the seventh volume of the second series of *Guy's Hospital Reports*. From his paper I have taken the liberty of copying the accompanying woodcut, which explains the steps of the operation.* Mr. C. H. Moore has also drawn attention to it in the *Medico-Chirurgical Transactions*. There are

* *Description of Fig. 6.*—A vertical section of the bones of the face; the inner surface of the lower jaw is partly hidden by the internal pterygoid and mylo-hyoid muscles; between these the gustatory nerve may be seen descending forwards.

A—Symphysis of the lower jaw.

B—Medial section of the os-hyoides.

C—Mylo-hyoid muscle, upper surface.

D—Internal pterygoid muscle, inner surface.

G N—Gustatory nerve marked at the spot where it should be divided.

many advantages to be derived from this operation besides the relief of pain; the flow of saliva is diminished materially, and this, as is well known, is a most distressing source of annoyance in malignant disease of the tongue. Again, patients often suffer in health, and become emaciated, not so much from the primary effects of the disease as from the unwillingness or even inability to take proper or sufficient food. The ulcer is not only painful, but sore to such a degree as to render the attempt at swallowing almost impossible, and in the case of solid food quite so. The division of the gustatory gets rid of this soreness; and the improvement in the patient's appearance after the operation is due in a great measure to his increased capacity for taking food. Again, if the tumour is to be removed, the previous division of the nerve is a positive advantage, inasmuch as the use either of knife, ecraseur, or ligature will then be almost painless. I have not, however, found the ecraseur so painful as to make it necessary to divide the nerve. Besides I find that chloroform is given just as safely in operations on the mouth as in other cases, and that it positively makes them easier of performance, the supposed difficulty from accumulation of blood and mucus not occurring. I now use it even in operations on the cleft palate, and find it facilitates the operation most materially. Still the advantages of dividing the gustatory in cases of malignant disease of the fore-part of the tongue are very manifest; and I think so simple a palliative ought to be more frequently adopted.

I have performed the operation five or six times; it is sometimes a little difficult to hit the nerve, in cases where the tongue is much swollen, but by a little management I have always succeeded. The relief afforded by it lasts to the full for five or six weeks, and in a slighter and gradually decreasing degree for two or three weeks more. By that time the divided nerve becomes again capable of transmitting the nervous current. The operation may then be repeated, if the disease has not by that time extended too far back, as will be most likely. I have been asked why I did not cut out a piece of the nerve. In the first place that would make the operation tedious, painful, and probably very difficult; the nerve should be exposed, and seized with forceps before a piece could be excised, while simple division of it in the way described above can be done without seeing it. Secondly, the interval taken by the nerve to unite and become perfect as a channel of sensation is about equal to the time which it takes the disease to spread to parts supplied by the glosso-pharyngeal nerve. Still, if a case occurred to me where

the disease was slight but painful, and where the patient refused to allow the removal of the disease, I should advise the excision of half an inch or so of the nerve.

ART. IX.—*Ophthalmoscopic Notes.—On the Theory and Principles of the Ophthalmoscope.* By HENRY WILSON, F.R.C.S.I.; L.K. and Q.C.P.I.; Assistant Surgeon to St. Mark's Ophthalmic Hospital; Member of the Royal Irish Academy, &c.

IN proportion as our knowledge of the laws of nature increases and as science advances, do the art and means of discovering the seat and nature of disease become more and more developed. The application of the exact sciences in the domain of medicine has resulted in the discovery, or the utilization, of chemical tests and mechanical contrivances whereby the practitioner is enabled to pronounce definitely on disordered conditions, the existence or nature of which he could only have guessed at, without the employment of those means. Morbid changes in the tissues and fluids, such as amyloid degeneration (which has been elucidated by Dr. Robert M'Donnell's able researches), *leucæmia*, &c., with which we were previously unacquainted, have been revealed by the microscope, by chemistry, &c.; diseased conditions of the vocal and respiratory apparatus hitherto unknown, except, perhaps, as *post mortem* appearances, have been not only recognized during life by means of the laryngoscope, but also successfully treated; quite recently the endoscope has been so perfected by my friend Dr. Cruise, as to bring into view the interior of the urethra, bladder, &c.; and the ophthalmoscope has opened quite a new field of investigation. Immediate exploration by instrumental aid, wherever practicable, is gradually superseding, in a great measure, all other methods of investigation, and the practitioner is becoming less and less dependent upon the statements of patients or their friends, which, owing to various causes, are in many instances so very unreliable; thus the art of diagnosis is becoming facilitated and perfected, and in direct ratio to the accuracy of diagnosis will the practice of medicine be less and less empirical.

Since Lænnec's great discovery of auscultation and the stethoscope, no instrument has rendered such vast services as the ophthalmoscope, not merely in the interpretation of purely ocular affections, but also

in the elucidation of disease in other organs. It renders visible in the living eye the various phenomena of healthy and diseased circulation, and many disordered conditions which illustrate and explain physiological and pathological processes elsewhere; by means of it morbid actions and structural changes may be watched from day to day, or hour to hour, and its employment not unfrequently leads to the detection of disease in other organs, such as the brain, kidneys, circulating apparatus, &c.

With the discovery of the ophthalmoscope a new era dawned in ophthalmic literature—discoveries followed each other in rapid succession—optical defects, the nature of which had been hitherto unknown, or misunderstood, have been explained and remedied, and the laws and mechanism of refraction and of accommodation have been but lately satisfactorily explained. In no branch of the profession, indeed, has there been such great progress of late years, as in ophthalmology, and, thanks to the indefatigable and zealous labours of earnest and scientific men, ophthalmic medicine is becoming more closely allied to the exact sciences; it is worthy of note, indeed, that some of the most distinguished oculists of the present day are eminent also as mathematicians.

The importance of the ophthalmoscope as a means of diagnosis can scarcely be overrated. I need only adduce in proof thereof the former definition of *amaurosis*, “that condition in which the patient sees nothing, and the physician likewise nothing;” this definition is now quite untenable, for the instrument renders either positive evidence, and the physician is able to see the cause of the blindness (which is the case in the large majority of amaurotic persons), or else it affords negative evidence, that is of the eye itself being healthy, and the physician is in a position to pronounce the cause of blindness (*amaurosis*) extra-ocular, and he may thereby be led to ascertain the nature and seat of the lesion or disturbance (generally intra-cranial) which has given origin to the blindness.

The term *amaurosis* is becoming less and less frequent, and will, I hope, eventually come to be used only in its true significance, and simply denote a symptom, and not as is but too generally the case at present, a disease.

And if by its means we are enabled to make a correct diagnosis, the ophthalmoscope must, of necessity, be useful also in the treatment of disease; while in one instance we are able to pronounce our patient incurable, and spare him the sufferings, mental as well as physical, of futile treatment—in another we are able to declare him

curable, and subject him to appropriate treatment. No doubt long experience and accurate observation, will, in a great number of cases, enable the practitioner to arrive, empirically, at the same conclusions, without the aid of the instrument, but how much more satisfactory to ourselves, and beneficial to the public, when even the youngest practitioner can at once give a prognosis as reliable, if not more so, than that which it has taken a lifetime, perhaps, to be enabled to pronounce.

The ophthalmoscope is especially important and useful in cases of suspected deception in the selection of recruits, for the various public services, or in the examination of those already in such services. Thus, defects can be detected in the eyes of recruits which would unfit them for various duties, and soldiers and others who are really labouring under disease of the internal structures of the globe, the eye presenting, externally, no sign whatever of disease, are now free from the odium which may have formerly attached to them as schemers, while real malingerers are liable to detection and punishment. The Army Medical Department has taken up the subject of the ophthalmoscope very warmly, and there is a special course of study thereon, at Netley, under the able guidance of its distinguished Professor of Military Surgery, Deputy Inspector-General Longmore. This increased attention to the vision of recruits, soldiers, &c., must result in a considerable pecuniary saving to the country at large. The following extract from Dr. Longmore's *Ophthalmic Manual*, published for the guidance of army surgeons, is, I think, equally applicable to civil as to military practice:—

“A soldier who is really the subject of serious disease may, by chance, be regarded as a malingerer, because all the superficial ocular apparatus are free from disease, and all that can be seen by ordinary observation of the deeper structures of the eye are observed to act precisely as they do in eyes that are known to be healthy. We have, unfortunately, conclusive proofs that such errors as these formerly occurred very frequently; and that they led to soldiers being sent on foreign service, often to tropical climates, where their diseases became rapidly aggravated, so that they had shortly to be invalided home again, as well as to the retention for considerable periods at home stations of others who were useless, and who had at last to be discharged from the service, after much expense being incurred by their prolonged maintenance. If a defect of vision really exists the medical officer should be able to report to his professional superiors, the exact cause of the disability, for on this

also may hinge many points of importance to the public service, to the soldier, and to the State. If the defective vision be the result of some natural abnormality of the refractive media, or special form of the globe, the service has suffered from the admission into its ranks of an ineligible recruit. If the defect be the result of disease, such as too often follows service under the glare of a tropical sun, then the soldier has suffered from his employment by the State, and is entitled to compensation according to the degree of his disability. There is only one way in which a diagnosis, in these cases, can be truly arrived at, and that is by means of the ophthalmoscope." ^a These, and other considerations, point to the very great practical utility of the instrument, the cost of which is trifling, and the manipulation not difficult; I would, therefore, claim for it a larger share of the attention of the Irish profession than has been, I think, hitherto accorded to it.^b

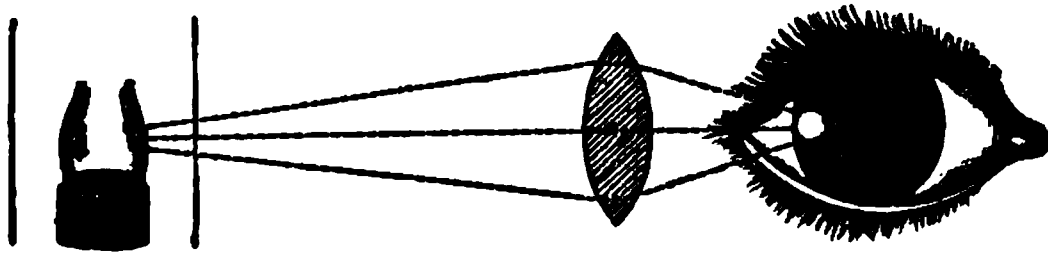
Before, however, entering upon the subject of the ophthalmoscope, I would direct attention to other methods of illumining and examining the eye. In the ophthalmoscopic examination we illumine the bottom of the eye, as shall be presently explained, by light *transmitted* through the transparent media; hence, any opacities in the latter will intercept some of the light, and appear simply opaque, as happens also in microscopic examinations; to determine the colour and character of these opacities, therefore, *incident* light must be employed, and this is effected by means of *focal* or *oblique illumination*. Focal or lateral illumination consists in concentrating, on any given spot of the eye, by means of a convex lens, the light from a lamp placed at the patient's side. I am in the habit of using for this purpose a two-inch convex lens. The apartment should be darkened, and a lamp placed about one foot and a half from the patient's side and towards the front, so that the flame and eye shall be nearly on a level, and the globe to be examined illumined, while the opposite eye remains in darkness; the convex lens is then held

^a One of the most able and exhaustive reports on ophthalmic surgery which has appeared of late years is that contained in the Army Medical Reports for 1860, written by Assistant-surgeon Dr. Frank, the merits of whose writings and teachings are universally acknowledged. Dr. Frank has withdrawn from the army, and it is much to be regretted that the State has thereby lost the services of so accomplished and efficient a medical officer. The greater part of his report is devoted to the examination of the deep-seated diseases of the eye, which can be recognized and studied only by means of the ophthalmoscope.

^b The various forms of ophthalmoscopes, as well as lenses and other optical appliances, can be had at Mr. Yeates', 2, Grafton-street.

between the thumb and index finger, two inches from the eye, as indicated in the woodcut, in such a manner that the rays from the

Fig. 1.



flame shall pass through it and be brought to a focus on the cornea. If it be desired to examine the iris, capsule, or lens, the convex lens must be held nearer to the eye so as to focus the rays on these parts. This mode of examination can also be practised by intercepting and concentrating on the eye, by the same lens, direct beams of sunlight; but the eye is, as a rule, not very tolerant of such illumination, and sunbeams are not always at command; ordinary diffused daylight does not suffice for the purpose. Another method of effecting *oblique illumination* consists in reflecting on or into the eye, by means of a mirror, the rays from a lamp, while the observer places his eye nearly in the line of these rays. This is best accomplished by holding a small circular plane mirror, or, better still, the ordinary ophthalmoscope mirror itself, just over or in front of the eyebrow, so that the rays from a lamp, placed at the side or above and behind the head of the patient shall be reflected into the observed eye. The distance between observer and patient is then diminished or increased, and slight rotatory movements of the mirror on its own axis executed until a satisfactory illumination is obtained of the part to be examined. Although it is only of recent years that these methods have received that attention which they undoubtedly deserve, similar modes of examination have long been known, and occasionally employed. Mackenzie mentions, at p. 508 of his *Practical Treatise on the Diseases of the Eye*, published in 1840, the circumstance of his having seen what he considered the effects of hyaloiditis, "in directing the light of a gas jet through the pupil with a lens;" and at p. 639 he speaks of concentrating the light on a cataractous opacity by means of a double convex lens. Himly in his *Krankheiten und Missbildungen des menschlichen Auges*, 1843, likewise recommends the ordinary daylight to be concentrated on the eye by means of a convex lens; he also advises illumination by means of reflected light from a mirror, but says it should be used only with completely blind eyes. He noticed a greyish or greenish appearance of the pupil when the interior of the eye was illumined,

and states that the blackness of the interior always ceases when strong illumination is used. It is worthy of note how very near to the discovery of the ophthalmoscope this author had arrived. By focal or oblique illumination each portion of the *cornea* may be brought successively into view. Opacities and irregularities in or on that membrane are exhibited with surprising distinctness; the effusions in *iritis*, covered with red vessels; the sequences of inflammation, such as adhesions, patches of pigment on the capsule of the lens, &c., are brought very clearly into view; their colour, extent, and consistency, are incomparably better ascertained by focal than by any other mode of illumination, and we are thus enabled to estimate the length of time which may have elapsed since the occurrence of the active inflammation. Partial or total loss of vision may occasionally form the subject of a medico-legal inquiry, and any iritic complication which might exist would afford valuable information. But it is principally in examinations of the *crystalline lens* and its capsule that I would advocate these methods; for the determination of the condition of these parts I rely altogether on focal illumination. The anterior capsule affords, as a rule, in health, a very faint reflection, but in young persons there should be no reflection from the interior of the lens, which should hence be invisible. The lens itself is composed of such various elements—tubes, fibres, &c., &c.—and is of such complicated arrangement, that it can be perfectly transparent only when all its component parts exhibit exactly the same refractive index. About the middle period of life (thirty), the individual parts begin to undergo an alteration in their refractive condition, and reflect a certain portion of the incident light, and consequently become visible. This fact should be always remembered, lest the natural reflection peculiar to advanced age be mistaken for cataract. If, when this reflection is strongly marked, the lens is examined by transmitted light, by means of the ophthalmoscope mirror, it will often be found perfectly transparent.

The seat, extent, and consistence of lenticular opacities may be accurately determined; and this is, in practice, an object of the greatest moment, for a knowledge of these circumstances will, as a rule, enable us to decide whether the opacity is a stationary or progressive one, or what length of time may elapse before the lens becomes completely opaque, and will also guide us in the selection of an operation. Opaque capsules, exudation membranes, &c., resulting from cataract operations, are best seen by focal illumination; I have been thus enabled, in some instances, to observe accurately

false membranes which I was not able to perceive otherwise, although I possess very acute vision. These cobweb-like glistening membranes are of a greyish colour, with whitish bands or specks scattered through them, or they appear as a cellular network of fine tendinous bands in one or two layers, stretching across the pupillary area, about midway, sometimes between the iris and vitreous, or simply in the pupillary opening of the iris. The impairment of vision caused by these membranes is often very great, and the improvement resulting from their removal is truly astonishing. Partially or completely luxated transparent lenses are also easily recognized by this illumination; the periphery or equator appears, as also in daylight illumination, as a brilliant amber or golden-coloured semicircle, varying in its position as the eye or light vary in their relative positions. This appearance is said to have been observed in the normally placed lens, in albinos, through the iris, which is scarcely at all pigmented; the periphery of the lens can be seen also where the iris is partially or wholly absent. Focal illumination allows us even to examine the anterior part of the *vitreous humour*, and affords the best means of seeing tumours protruding into the chamber of vitreous humour.

The luminous appearance presented by the eyes of certain animals, such as the dog, cat, &c., occupied the attention of philosophers from a very early period. At the commencement of the last century Méry made some investigations on the subject, and while examining a living cat's eye, under water, saw the retinal vessels. This was subsequently confirmed by Lattère, who accounted for it by supposing that the eye under water adapted itself to the new medium, and that the rays, leaving the eye, were divergent, and therefore brought to a focus on the observer's retina, a theory we now know to be fallacious. Based, however, on the fact of the interior of the eye being visible under water, is Czermak's *orthoscope*, which is a box-like apparatus with glass sides, open at one side so as to fit over the eye, and at the top for the reception of water; when fitted on to the integuments, over the orbital margin, it forms a water-tight chamber, projecting into which is the eyeball. The water in front of the cornea acts as a lens, and the natural focus of the eye is displaced forwards, and hence the retinal vessels become visible.

At the beginning of this century Prévost established the fact that the luminosity of animals eyes was not perceptible in a *perfectly* dark apartment, but that some light must be present; he concluded, therefore, that the luminosity was attributable to a *reflection* from

the bottom of the eye, of light which had penetrated from without. He also found that this luminosity could be seen only in certain positions, and not in others. In the dog and cat, and indeed in the majority of the carnivora, in the ruminantia, in the horse, in the cetacea, and in the cartilaginous fish, there exists at the outside of the optic nerve entrance a patch of peculiarly constituted choroid, which is devoid of pigment, and of a beautiful metallic lustre. This is the *tapetum lucidum*; it varies in colour—blue and green being the predominant ones—and has a smooth polished surface, which acts as a concave mirror. The luminosity in these animals is attributable then, not as was anciently supposed, to spontaneous generation of light, &c., but to a reflection of light incident on this tapetum.

The blackness of the human pupil was referred to the complete absorption of light by the choroid membrane which possesses no tapetum. Complete absorption of light is, however, very rare; were the choroid even as dark as charcoal (one of the substances most capable of absorbing light), a portion of the rays would still be reflected, for in their passage from one medium to another rays of light are never so completely and altogether absorbed but that some of them are reflected. Moreover, certain portions of the healthy retina, such as its vessels, as also the intraocular end of the optic nerve, should reflect a large portion of light. The blackness of the pupil cannot, therefore, be owing to absorption of light. Furthermore, we know from practical experience that although the pupil is, under ordinary circumstances, black, yet, it has occasionally been observed to be luminous; thus, I have seen the human eye as brilliant as any animals eye, on the occasion of a laryngoscopic demonstration, where all the circumstances of Cumming's experiments, to be mentioned presently, were accidentally fulfilled; similar occurrences have been noticed by other observers. The interior of the human eye was long ago noticed to be luminous in albinos, in partial or complete deficiency of the iris, in certain diseased conditions, such as fungus hematodes, various tumours, separation of the retina, &c.; the "cat's-eye amaurosis," first described by Beer, at page 495 of his *Augenkrankheiten*, in which there existed in the bottom of the eye a "concave pale grey or yellowish-white, or reddish opacity," was, I believe, in the majority of instances due to separation of the retina. A consideration of these theories and facts leads to the conclusion that a portion of the light which enters the eye is again reflected from it, but that we are, under ordinary circumstances, unable to perceive it.

One of the first to investigate this subject, and to arrive at some satisfactory conclusions, was Mr. Cumming, formerly house surgeon to the London Hospital; it may be said, indeed, that on his experiments and writings is based the discovery of the ophthalmoscope. In 1846 he published, in the twenty-ninth volume of the *Medico-Chirurgical Transactions*, his essay^a “on a luminous appearance of the human eye,” the object of which was “to show that the healthy human eye is equally, or nearly equally, luminous as the eye of the cat, dog, &c., when observed under favourable circumstances; and the application of the abnormal appearance, or want of this luminosity to the detection of changes in the retina and posterior part of the eye.” The circumstances necessary for observing this luminosity are, he remarks:—“*a.* That the eye must be at some distance from the source of light, the distance being greater in proportion to the intensity. *b.* That the rays of light diffused around the patient (and sometimes around the eye itself), should be excluded. *c.* That the observer should occupy a position as near as possible to the direct line, between the source of light and the eye examined; hence, it is sometimes necessary for the observer to stand obliquely, that his eye may approach nearer to the direct line.” “Let the person under examination sit or stand eight or ten feet from a gas-light, looking a little to the side; standing nearer the gaslight we have only to approach as near as possible to the direct line, between it and the eye to be viewed, at once to see the reflection.” It having thus been established that the human eye is, under certain circumstances, equally luminous as that of animals, the enquiry arises what these circumstances are, and how we may at all times, and most effectually for practical purposes, illumine the interior of the eye.

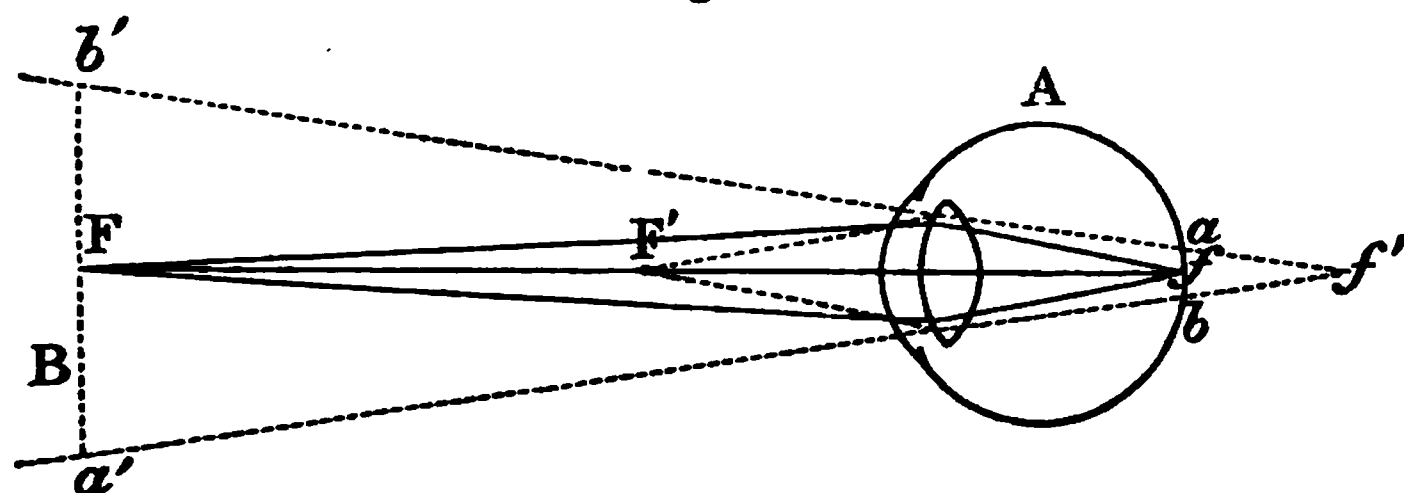
The human eye possesses a dioptric apparatus, and is endowed, in addition to the ordinary refractive powers of its system of lenses, with the vital function of adjustment—altering its focus, and thus accommodating itself for various distances and various sized objects. By means of this *accommodation* we are able to see an object distinctly whether it be one foot or six feet distant. Various have been the explanations of the mechanism whereby this is effected; some imagined it to take place by alteration in the shape of the globe by means of the extrinsic muscles of the eyeball. Kepler, the celebrated mathematician of the 16-17th century, who

^a See Sir Wm. Wilde's notice thereof in the number of this Journal for February, 1847, p. 257.

first described the humours of the eye as refractive media, and the consequent formation of images on the retina, was also the first to ascribe this accommodation to the ciliary ligament, the contraction of which produced, according to him, elongation of the globe, in which movement the retina was carried to a greater distance from the lens when objects were near. Porterfield, a learned physician and mathematician, whose *Treatise on the Eye—the Manner and Phenomena of Vision*, 1759, is still one of the best works we possess, regarded the ciliary ligament as muscular, and attributed to it the function of changing the position of the lens, of increasing or diminishing the distance between it and the retina, according to the different distances of objects to be seen, so that the retina should be “always at a due focal distance behind the crystalline.” According to a still older theory, the ciliary ligament or muscle effected an alteration in the curvature of the lens, and hence in its focus; and this theory has been, in recent times, amply confirmed by the experiments of Helmholtz, Knapp, and others.

A normal eye is so constructed that when at rest parallel rays striking the cornea are brought to a focus on the bacillary layer of the retina (Jacob's membrane). Strictly speaking, there is in nature no such thing as parallel rays, for all rays emanating from a luminous point are divergent and must continue so; the divergence of rays coming from an infinite distance is, however, so very slight, and the surface of our eye, on which they impinge so very small, that practically we may look upon them as parallel. If an eye be accommodated for a luminous object—say a flame—rays from it on entering the eye will undergo a certain definite refraction or change of direction whereby they are brought to a focus on the retina; a portion of these rays will be absorbed by the choroid, while the remainder will be reflected; this reflected portion must, however, return from the bottom of the eye in exactly the same line as it entered; it will therefore again undergo a refraction similar in nature, but opposite in direction, to that it underwent on entering; and emerging from the eye these rays will return to the flame from which they originated, and there form their focus. Let A (Fig. 2) be an eye accommodated for F, a flame; rays from the latter impinge on the cornea slightly divergently, and entering the eye will be rendered convergent by the refractive media and brought to a focus at f , at which place there will therefore be an image of F; from this luminous image rays will be reflected outwards in the same line as they entered; they strike the posterior surface of the crystalline

Fig. 2.



lens divergently, are converged by it, and emerge from the eye convergently, and are united into a focus at F ; hence, F , the luminous object and, f , its retinal image will act reciprocally as *conjugate foci*, each being alternately object or image—whichever be the radiant point the other will be the focal point. In like manner, when we regard another individual's pupil, it will be merely a representation of our own; and as our own pupil appears black, and sends no light into the observed eye, the pupil of the latter will consequently appear black also. The eye of the albino would appear, at first sight, to offer a contradiction to the above, as the pupil in such cases presents, even in ordinary daylight, a reddish or brilliant appearance, but this luminosity is due to rays entering the eye through the very scantily pigmented iris and choroid, parts which ordinarily possess such an abundant pigmentation as to render them very opaque. This may be proved to be the case by holding a blackened card or screen with a central aperture corresponding to the pupil in such a manner before the eye that all rays except those entering through the pupil shall be excluded; the red reflection will then disappear, and the pupil appear black like that of an ordinarily pigmented eye.

In order then for an observer to perceive the returning rays his eye must be in the line of their emergence, *i.e.*, between F and A , or at F , for at any other place (such as at B) A 's pupil will appear black. If we interpose between F and A the illuminating rays would be necessarily cut off; and it is impracticable to place our eye at F . We may, however, bring our eye very close to F , and by looking through the margin of the flame we approach very nearly to the direct line of the emergent rays, and in this position we obtain a bright reflection from behind A 's pupil, as in Cumming's experiments.

Professor Helmholtz, now of the University of Heidelberg, constructed an apparatus by means of which the observer could readily

place his eye in the direct line of the emergent rays; and in 1851 he published an account thereof. The chief features in this invention are the employment of *reflected* light, and the utilization of the laws of refraction. Let A, in Fig. 3, represent the observed

Fig. 3.



M

eye, and F a flame on the same level, five inches to the side, and a little towards the front of the patient; M, a piece of well polished plane glass, with parallel surfaces, placed one inch in front of A, at such angle that rays falling on it from F shall be reflected into the eye, as if they came from F', which is a point as far behind M as the flame is in front of it. The observed eye will apparently see the flame at F', for which distance (five inches) he then accommodates. The plate M acts as a speculum, but being transparent some of the rays from F pass through it, and are lost; others, however, are reflected into A, and are brought to a focus on its retina—at *a*—where there will be consequently an image of F. As already explained rays emerge from this image, some of which are reflected back to F by M, while the remainder traverse M to form their focus at F', the point for which the eye is accommodated, and where there will therefore be an image of *a*. The observer, therefore, can now, by placing his eye, B, behind M, between it and F', readily intercept these rays, but yet he does not receive on his retina an image of *a*, but merely perceives a brilliant red reflection from A. The cause of this lies in the fact of the rays *a* M F' being

convergent, and being still further converged by the dioptric apparatus of B (a normal eye) they unite into their focus between the lens and retina at *o* (indicated in Fig. 3 by the interrupted lines), where they become divergent and form a circle of dispersion, *c d*, on B's retina—hence there is no distinct image of *a*. By interposing a concave lens, *L*, however, between M and B these convergent rays are rendered divergent (indicated by the dotted lines), and entering B are again converged by the refractive media, so as to be focussed on B's retina at *a'*. The observer, B, will therefore now obtain a clear and well-defined image of the illumined spot *a*. No matter where B placed his eye so long as the rays from A were convergent he could only obtain a confused and indistinct image. This simple contrivance was improved by the addition of other plates of glass, which, together with suitable lens, were fitted into an appropriate tubular case, and was styled by its inventor *speculum oculi* or *ophthalmoscope*, and is no doubt the first ophthalmoscope, properly so called, by means of which the retina of the living eye could be seen. The subject was, I believe, first brought before the English profession by Dr. Sanders, in the *Monthly Journal of Medical Science* for July, 1852. Professor Wharton Jones announced in the *British and Foreign Medico-Chirurgical Review* for October, 1854, that Mr. Babbage had, seven years previously, showed him the model of an ophthalmoscope which “consisted of a bit of plain mirror, with the silvering scraped off at two or three small spots in the middle, fixed within a tube at such an angle that the rays of light, falling on it through an opening in the side of the tube, were reflected into the eye to be observed, and to which the one end of the tube was directed. The observer looked through the clear spots of the mirror from the other end.” It is much to be regretted, for the sake of British ophthalmic surgery, that this discovery was not made public, or even utilized by the learned professor of ophthalmic medicine and surgery in University College Hospital, London, at an earlier period.

By means of Helmholtz's ophthalmoscope, just described, the image of the flame could be projected *successively* on different portions of the bottom of the eye, and thus gradually an examination of the whole fundus^a might be obtained. Although the instrument was subsequently much improved by Epkens, by its deviser, and others, it was always difficult to manipulate, and the

^a By the term *fundus* is meant all that part of the eye lying behind the lens and vitreous humour, viz., optic nerve entrance, retina, choroid, sclerotic, and blood-vessels.

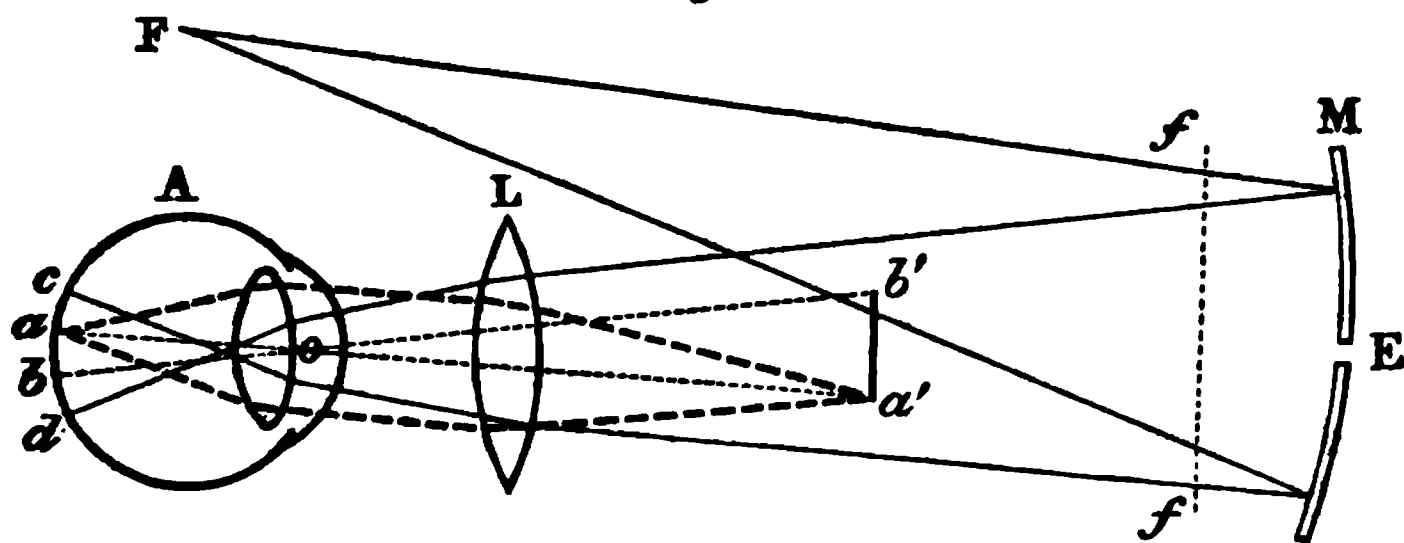
field of illumination being so very small, it was of but little practical value.

As I have endeavoured to explain, the illumination consisted in bringing to a focus exactly on the retina the image of a flame. Another method, conceived by Professor Brücke, consists in illuminating the fundus by means of a *circle of dispersed light*. In 1847, Brücke published an account of his experiment, whereby the illumination of the interior of the eye could be easily and satisfactorily demonstrated. This experiment, which is based on a knowledge of the laws of refraction and accommodation, already briefly alluded to, consists in adjusting the vision for a shorter or longer distance than that occupied by a flame placed in front of the eye, so that instead of an image of the flame there shall be merely dispersed light on the fundus. While the observed eye A (Fig. 2) remains adjusted or accommodated for the distance A F, move the flame nearer to the eye—to F'; its image would now fall at f' , but that the retina is interposed; therefore a part of the fundus $a b$ will be illumined by diffused light. From this circle of dispersion $a b$, rays are reflected outwards which must return, and form their focus, not in the flame from which they originated, but at the distance for which the eye is accommodated—F, at which place there will consequently be an image $b' a'$ formed of $a b$. If the observer's eye now occupy a position anywhere between b' and a' (behind and to the side of F), the returning rays are readily perceived, the luminosity of A's pupil becomes very apparent, and even the details of the fundus are occasionally seen. This image $b' a'$ is formed at a considerable distance from the observed eye, is inverted, large, faint, and indistinct, and the details, as a rule, not recognizable; hence, though perfect as a means of rendering the illumination visible, it is, as a means of seeing the individual parts, practically useless.

To the acute and reasoning mind of Helmholtz, however, this experiment suggested a practical idea, and in his hands assumed a new and great importance, for from it he deduced and perfected a method of obtaining easily a well-defined image of the fundus. This method consisted in projecting on the fundus a large circle of dispersion, and converging the reflected rays to a focus at a short and convenient distance, by means of a double convex lens of short focus held before the eye. Rays of light falling on this convex lens are refracted, and enter the eye convergently; they are still further converged by the crystalline lens of the

eye and brought to a focus somewhere between this and the retina; from this focus they diverge, and fall on the fundus in the shape of dispersed light; from this circle of dispersion rays are reflected from the fundus, and emerge parallel if the eye be accommodated for parallel rays (as a rule, however, they emerge slightly convergently). Meeting the convex lens in front of the eye, they are converged and united within the plane of the principal focus of the lens, at which place an image of the fundus is formed. The flame of a candle or lamp placed before the eye was used for the illumination, and a screen was fixed behind the flame, so that the observer was enabled to bring his eye close to the source of illumination, and thus be as nearly as possible in the direct line of the emergent rays. To obviate the inconveniences and difficulties attendant on this mode of illumination Professor Ruete had recourse to *mirrors*. Concave mirrors possess the property of rendering rays falling on their surface from a certain distance convergent; therefore in them we possess a means of producing a circle of dispersion in the retina. Fig. 4 illustrates the passage of rays to and from the fundus.

Fig. 4.



Let *A* be the observed eye, accommodated for distance, *i.e.*, for parallel rays, *o* its optical centre, and *F* the flame placed behind or to the side of *A*, so that the cornea and pupil may be in darkness. A concave mirror, *M*, of suitable focus is now held in front of *A*, so that rays falling on it divergently or parallel from *F* are reflected into the eye convergently, and illumine the fundus as dispersed light; from this circle of dispersion rays are reflected outwards to form at *f*, *f*, as already mentioned, a large inverted and indistinct image of the fundus; if we now interpose between *A* and *M*, at its focal distance from *A*, a double convex lens *L*, of short focus—two inches—the rays from *M* will be rendered more convergent by *L*, and still further so by the dioptric apparatus of *A*, so that they will

unite behind the lens of the latter and form on the fundus a circle of dispersion $c d$; from any points of this circle, such as from $a b$, pencils of rays will be reflected and emerge from the eye parallel to the lines (dotted) through the optical centre; they are, however, converged by L , and brought to their foci at $b' a'$, at which place, then, there will be an inverted, magnified, and distinct image of $a b$ (the interrupted lines indicate the course of rays from a , and similar ones may be traced from b). By making an opening in the mirror at E , and placing our eye there, and accommodating our vision for the distance $E b' a'$, we readily become cognizant of the image. As is evident, this is an *aërial image situate between the observer and the convex lens*; it is also actually *inverted* and enlarged. If it be desired to obtain an enlargement of this image, it may be effected by placing behind the mirror another convex lens, but of a longer focal distance than L ; or we may primarily produce a large image of $a b$ by having L of a long focal distance, such as four inches, for such a lens will be of a lower converging capacity than one of two inches; and hence the image will be larger. By the above process we illumine the greater part of the fundus, and by using a convex lens of very short focus we obtain a very large field, *i.e.*, a view of nearly the whole illuminated portion, but the details are very small; by using a lens of longer focus the field of vision is curtailed, but the parts much magnified: thus, for instance, if we employ a lens of two-inch focus we obtain an image of the entire fundus, whereas if we use one of four we merely obtain an image of the optic nerve entrance. This mode of examining has been termed the *indirect*, or the examination of the reversed aërial image, to distinguish it from another, styled the *direct*—in which a *virtual erect* image of the fundus is seen.

ART. X.—*On the Use of Sulphite of Magnesia in the Treatment of Zymotic Diseases.* By H. R. DE RICCI.

It is now upwards of two years since I last published, in the pages of this Journal, the results of my investigations in the treatment and prophylaxis of zymotic diseases by the administration of the alkaline and earthy sulphites, and especially by the sulphite of magnesia; but, although my pen has so long been silent on this highly interesting and most important branch of therapeutics, it is not because I

have relaxed from my labours, for I still have diligently pursued my investigations, administering the sulphites on every occasion in which I thought they would be productive of good, and carefully noting the effects. I have also collected, from different sources, the results of sulphitic treatment, both in Europe and in other parts of the world, and in the hope that this information may eventually be of service to medical science, I beg to lay it before my professional brethren.

I regret to find, upon enquiry, that up to this date the treatment by sulphitic salts has found but few disciples in these islands, a fact much to be regretted, as those few who have tried this remedy in appropriate cases have almost invariably been satisfied with the results. No doubt the treatment has been in many cases unsuccessful, often in consequence of the administration of wrong preparations; and, besides, I do not presume to assert that every case of pyemia, puerperal fever, scarlatina, or other zymotic affection, will be cured by the administration of sulphite of magnesia, but I do assert that a great majority of such cases will recover under this treatment, if the remedy be only taken sufficiently early. I say, if taken sufficiently early, and it will be easily understood why I insist on early treatment, because, if in a case of zymotic disease, of whatever species, the treatment is delayed until the blood is so loaded with poison, so deteriorated in quality, as to be no longer able to perform its normal functions; such a case, if treated by sulphites, is no more likely to recover than if treated on any other principle, or without any principle at all, or let alone to the *vis medicatrix naturæ*; the blood being thoroughly poisoned is incapable of maintaining life; and though the sulphites may be administered, and though they may arrest the further propagation of the poison by neutralizing or by rendering the blood incapable of being infected, still they can never restore to life the defunct blood corpuscles; and if, in a given case, the destruction of these most important organisms reaches an amount such as to leave an insufficient quantity of healthy blood corpuscles to carry on the normal functions of organic life, death must be the result. If, for instance, we assume that at least 50 per 100 of normal blood corpuscles, is the minimum quantity which will enable an animal to exist, and that in a given case of zymotic disease the destruction of these corpuscles reaches 60 per 100, such a case, even though saturated with sulphites, would of necessity end fatally, because, although the existing poison would be neutralized, and its further increase put a stop to, yet the

amount of healthy blood remaining would be insufficient for maintaining life, and death would be the inevitable result. Hence the importance of early treatment, of early administration of sulphites, while still a large portion of the blood is in a healthy state, and capable not only of carrying on life, but also sufficiently strong to throw off the poison, now rendered inert by the presence of the sulphurous acid. To the tardy administration of the sulphites I, therefore, in part ascribe their want of success in many cases in which they have been tried in these countries. Another cause of failure I attribute to the administration of *hyposulphite of soda*—instead of the sulphites, and especially the sulphite of magnesia—which, on enquiry, I find to have been the salt most frequently employed. I have long ceased to employ the hyposulphite of soda, and for the last two years have invariably administered the sulphite of magnesia; and this for three reasons:—Firstly, because when administering hyposulphite of soda the greater portion of the acid becomes oxydized, during its progress through the animal economy, and appears in the urine as a sulphate; Secondly, because it is a salt of hyposulphurous acid, and, as such, a less active anti-zymotic; and Thirdly, because it often causes troublesome diarrhea, while the sulphites of soda and magnesia never produce such effects; now, of these last two, I prefer the magnesian salt, and employ it exclusively for internal administration, as, not only, it is less unpalatable, owing to its insolubility, but in consequence of the atomic weight of magnesia, it contains bulk for bulk more acid than the soda salt; and, as the acid is the active ingredient, it stands to reason that the magnesian salt must be the most efficacious of the two; the sulphite of soda I employ, almost exclusively, for external applications, because, in consequence of its greater solubility, a much stronger lotion can be made with it than with the magnesian salt. The sulphites of potass, ammonia, and lime, are also active anti-zymotics, but in no way superior to the salts of magnesia and soda, while their very noxious taste render them undesirable.

It is very much to be regretted that so few physicians have brought the value of these remedies to the test of clinical experiment, by which, after all, every remedy must be tried, and stand or fall by the result; as I several years ago asserted, this remedy is perfectly harmless; is not like arsenic, mercury, iodine, or strychnia, a double-edged instrument, equally able to save or to destroy; neither is it one of those remedies which can only be administered in fractional doses, and of which a grain more may endanger the life of

a patient; I have myself taken six drachms of it in twenty-four hours without any evil result, and I never have had to prescribe more than half an ounce of it in practice, even in extreme cases; I cannot, therefore, conceive why a more extended trial of its properties has not been made. Perhaps it may be thought by some that I exaggerate to myself the result of my treatment, and that I naturally feel biassed in favour of a remedy, that I have so prominently brought into notice during the last few years; be it so; let my experiments on animals be forgotten, and the results of my treatment of diseases in man be ignored, still the important paper of Dr. Cummins on scarlatina, *Dublin Quarterly Journal*, No. 77, p. 11; the observations of Dr. Waters in same Journal, and the paper on Diphtheria by Dr. Hayden in the same Journal, No. 83, p. 86, go far to prove (even if we had no other evidence), that we possess, in the sulphites, a powerful agent, whose action in the animal economy well deserves to be studied.

The following case will be read with interest:—Last Winter I had under my care a gentleman, over eighty, who was suffering from the most aggravated form of pompholyx diutinus I ever witnessed. The bullæ daily appeared all over his body, principally affecting the lower half, and varied in size from a millet seed to a hen's egg; when first they cropped out their contents were clear; but in twenty-four hours became turbid, then opaque, and the raw surface, which appeared when the bullæ burst, remained long unhealed, and discharged an acrid bad-smelling pus.

The patient had been for some months in this state, and was gradually getting worse, when he came up from the country and placed himself under my care; every variety of treatment having been tried, both locally and constitutionally, without obtaining any beneficial result, I determined, in consultation with Dr. Hudson, on placing the patient under a sulphitic treatment, in the hopes of arresting the further increase of the disease, and thus giving nature an opportunity of eliminating the poison. As the patient was so advanced in years I preferred to commence by administering the remedy endermically, as I wanted besides to observe what effect the remedy would have on the secreting surfaces. I therefore dressed the raw spots with pledgets of lint soaked in a saturated solution of sulphite of soda in water, adding a little glycerine to the lotion; the immediate effect on the sores was most remarkable; by the second day they had completely lost their unhealthy dirty look, and had assumed a bright rosy hue, and were quickly skinning

on the edges, and the pain caused by such an extent of raw surface was completely relieved by the application. At this time the extent of the sores was so great that scarcely a sound spot could be found from the hips to the soles of the feet. Being curious to ascertain whether the remedy was acting only locally or was also entering the system generally, I got my friend Dr. Davy to examine some of the urine of this gentleman, when he found that it gave ample evidence of the presence of *sulphurous acid*. After some weeks of treatment by external application, during which a very great improvement could be observed, both in the size of the bullæ and their daily numbers, I commenced the internal administration of the remedy in ten-grain doses every fourth hour, using this time the sulphite of magnesia; no bad result occurred—no diarrhea, no loss of appetite, no nausea, but a still greater improvement; the bullæ now appearing only at intervals of two or three days, never larger than half an inch in diameter, and only one or two at a time; while at the commencement, when he first came under my care, the average number of bullæ which daily made their appearance was from twenty-five to thirty, their size averaging one inch and a half in diameter. The patient was now so far improved as to be able to go out to drive every day, and to enjoy life, whereas during the previous few months his existence had been a burthen to him; unfortunately, one day, he exposed himself to cold, got influenza, of which at the time there was an epidemic in Dublin, and in a few days he died—a result not to be wondered at when it is remembered that influenza at the age of eighty-two is almost always fatal. Two instructive facts are deducible from the above case: first and foremost, that sulphite of magnesia does not disturb digestion nor cause diarrhea, both of which bad effects have been laid to the charge of this remedy; and, secondly, that the sulphites, even when applied endermically, are absorbed and circulated *as sulphites*, and finally excreted by the kidneys undecomposed, *at least, in part*. This is a very important fact to note, because it has been asserted that if even my theory were correct, the treatment by sulphites could not stand the test of experiment, as the sulphites, during their progress through the economy, become oxydized and converted into sulphates, even before they reach the circulation. This I have repeatedly proved to be incorrect; and though some portion of the salt, no doubt, does become further oxydized, a considerable portion will find its way unchanged into the circulation, as I have been able to detect the

presence of sulphurous acid in the urine even when very small doses of the salt had been administered.*

The next example I shall bring forward is a case of multiple abscesses depending on the absorption of a zymotic poison. A sailor, twenty-four years of age, of a not very robust constitution, but still not of a strumous habit, returning from a voyage in the West Indies, during which he had visited places where there was yellow fever, was attacked during the voyage home by sickness of anomalous character; he had shiverings occasionally, and at times some vomiting and diarrhea; the captain of his ship gave him some simple purgatives and other remedies, and he seemed at times to improve, but still the man complained of terrible lassitude, was considerably jaundiced, and after a short time was attacked with boils and abscesses; eventually he reached the port of Dublin, when he came under my care. I found, on examination, that he had whitlows on almost every finger, a very ill-looking semi-phagedenic ulceration of the glands of the groin, and several superficial sores on his legs and arms; his pulse ranged between 85 and 100, his bowels were relaxed, tongue coated, skin yellowish, and hot. Having carefully listened to his history I thought it probable that he had absorbed some animal poison, and that nature was busy eliminating it. I at once administered sulphite of magnesia, in scruple doses, three times a day, and dressed all the abraded surfaces with a saturated solution of sulphite of soda. After a week I increased the dose to one scruple, four times a day, and administered it in decoction of bark and compound tincture of bark; the result was most satisfactory; the sore in the groin became healthy-looking, the whitlows rapidly healed, and in three weeks he ceased to attend. Now in this case I am convinced that the man had been infected, while in the West Indies, with the poison of yellow fever, which from some cause or other having been prevented from pursuing its ordinary course, had contaminated the blood and given rise to those boils, whitlows, and abscesses, which were no more than the results of the efforts which nature was making to expel the poison; but as fast as nature expelled it at one point the poison

* Dr. Davy's mode of analysis is very simple and efficacious, and can be easily applied in a hospital ward. A large test tube being half filled with urine slightly acidulated with pure muriatic acid, a piece of starched paper stained with a weak solution of tincture of iodine, is suspended in the tube over, but not in contact with, the urine; if any sulphite is in the urine the iodine stain will be removed by the sulphurous acid gas which is evolved.

was re-producing itself in another, and eventually, in all probability, the patient would have sunk from exhaustion, but the presence of the sulphite which I administered checked the further increase of the poison, and then nature was enabled to effect a cure.

A short time ago I was attending a gentleman who was dying from phthisis senilis; his expectoration was distressingly profuse and was annoying him more than any other symptom; remembering the success which had attended the administration of a sulphite in a case of phthisis which I attended in 1862, I administered five grains of bisulphite of magnesia every third hour; the result was most remarkable; not only was the purulent expectoration reduced far beyond what I had expected, but the night sweats, which also troubled him excessively, almost entirely ceased; and though the medicine was unable to avert the final result, it was at any rate able to give ease and comfort to a dying man, which, next to saving life, is the great object of our mission in this world.

A few days ago a well-known French cook, in this city, came to me in great alarm about himself; he had given himself a very slight wound, a mere scratch with his knife, while dressing a piece of raw meat. I never saw a man looking more ill; he was jaundiced; he was so feeble as scarcely to be able to stand; his pulse was 100, his tongue foul, his bowels relaxed, and his wounded hand very much swelled, and excruciatingly painful. The wound though slight was dark and angry-looking, and red lines coursed up his fore-arm, following the tracks of the lymphatics. I wrapped up his hand and arm in lint soaked in a saturated tepid solution of sulphite of soda (which is much more soluble than sulphite of magnesia), and wrapped this again in gutta percha tissue, and administered sulphite of magnesia in ten-grain doses, four times a day. The improvement was rapid and manifest; the dressing had not been three hours on his hand and arm when he experienced the greatest relief from pain. In two days the swelling had almost entirely subsided, and in less than a week from the commencement of the treatment he was perfectly well. I have just stated that the application of sulphite of soda immediately relieved the pain, which at the time of my first visit was almost intolerable. I have had occasion in several cases to observe the extraordinary anesthetic property of sulphite of soda when applied to raw surfaces, especially in burns; and although I cannot well explain to myself the way in which this phenomenon occurs, still I have too often observed its recurrence to any way doubt the accuracy of my observation. I

had a case of very ill-conditioned sore throat under my care last Spring, which I feared might terminate in diphtheria. I administered sulphite of magnesia internally, and three times a day; I directed that half a scruple of the powdered salt should be insufflated by means of a quill into the pharynx, which last invariably produced the greatest ease from pain.

I have the notes of several other cases equally instructive and interesting, but I do not wish to weary my readers with useless repetitions; each case points in the same direction; and although each case was not a success, yet the disappointments have been few; and although up to this date both my theory and its application have found but few followers in this country, still I venture to predict that eventually the treatment of zymotic diseases by the administration of sulphites will be as fully recognized as that of ague by cinchona; nay more, for if paludal and intermittent fevers be the result of zymotic infection, as it is almost certain that they are, cinchona itself will be superseded by the sulphites both in the treatment and prophylaxis of those diseases.

ART. XI.—*Report on Scalds of the Larynx.* By PHILIP BEVAN, M.D., T.C.D.; F.R.C.S.I.; Surgeon to Mercer's Hospital.

IN the fifty-seventh number of this Journal, I published four cases of scalds of the larynx cured by the rapid exhibition of mercury; I now desire to record five other cases, in which the same treatment has been adopted with complete success. Two of these have been already published, by Dr. Croly, in the *Medical Press* of the 2nd May, 1866; one case occurred to Dr. Geoghegan, in the City of Dublin Hospital; one in Mercer's Hospital, to Dr. Ledwich; and another to myself in the same institution; so that, in nine consecutive cases the practice has been successful.

When we contrast this result with the excessive mortality which usually follows the treatment of the accident by tracheotomy it must be considered a most important improvement in practice. As to the fatality of the latter treatment it is difficult to arrive at anything like correct statistics, as the successful cases are usually published, whilst the unsuccessful ones are forgotten; but I think it will be easy to prove that it is one of the most fatal operations in surgery, when performed for this accident. I freely confess it

has been so in my own practice, and in that of many most eminent surgeons, whose cases I have had an opportunity of observing; and most of the trustworthy surgical writers acknowledge the same.

Thus Erichsen says, page 311, "in the majority of cases in which this operation has been performed the issue has been fatal;" and Gross, in his elaborate *System of Surgery*, Vol. II., p. 436, "that the operation is rarely of any permanent benefit is clearly proved by the statistics published on the subject, which show a sad disproportion of deaths to recoveries." Also in the able statistical report in the *Medical Times and Gazette*, nineteenth volume, New Series, p. 405, fourteen cases are narrated, with only three recoveries. Dr. Jameson records eleven cases, with only three recoveries.

Dr. Wilkes, in a most able report on diseases of children,^a publishes two cases of operation, both fatal; and Dr. Bryant in the same volume,^b publishes nine cases of the same operation, of which five were successful.

Statistics of scalds of larynx treated by tracheotomy:—

					Cases.	Deaths.	Recoveries.
Reported in <i>Medical Times and Gazette</i>							
(S. C.),	-	-	-	-	14	11	3
Reported by Dr. Jameson,	-	-	-	-	11	8	3
„	Dr. Wilkes,	in	Guy's	Hospital,	2	2	0
„	Dr. Bryant,		do.,		9	4	5
					<hr/>	<hr/>	<hr/>
					36	25	11
Treated by mercury without operation—							
By Dr. Croly,	-	-	-	-	2	0	2
By Dr. Geoghegan,	-	-	-	-	1	0	1
By Dr. Ledwich,	-	-	-	-	1	0	1
By Dr. Bevan,	-	-	-	-	5	0	5
					<hr/>	<hr/>	<hr/>
					9	0	9

Dr. Croly has proved by reference to numerous surgical writers that antiphlogistics followed by tracheotomy is the recognized practice for scalds of the larynx; but it was scarcely necessary to do so, as the statistics above referred to prove that it is the practice adopted in the leading hospitals of London.

My belief is, that tracheotomy for scalds of the larynx is based

^a Vol. vi., Second Series, Guy's Hospital Reports, p. 133.

^b Vol. vi., Second Series, Guy's Hospital Reports, p. 19.

on a false view of the pathology of the case. The croupy respiration during life, and the redness and swelling of the mucous membrane around the glottis after death, have led surgeons to suppose that all the disease was located in the larynx, hence in many cases the lungs have not been examined at all, or very superficially; but although, no doubt, the larynx is the part primarily injured, observation will prove that it is only the seat of a part, and not even the principal part of the effects of the accident. It is not easy to examine the chest of a child under such circumstances; on the one hand, they are cross, restless, and cry on any attempt to disturb them; and on the other, the loud croupy respiratory murmur renders it difficult to hear the chest sounds accurately; but if care be taken with patients, we will always be able to satisfy ourselves that the child is not dying of suffocation, and that sufficient air gets into the lungs, as long as those organs are capable of receiving it. Again, although *post mortem* examination of the larynx always shows redness and swelling of the mucous membrane, yet in any case I have myself examined there has been quite sufficient room for the ingress of air. In one case, seen many years since, of which I have notes, the mucous membrane was swollen, red, and puffy, but the rima glottidis sufficiently open to allow of respiration. Dr. Wilkes, on examining one of his cases, makes a similar observation. He says:—"The epiglottis and glottis were both swollen, but not to any great extent, so as to produce closure."

Dr. Bryant^a describes the *post mortem* appearances in one case:—"The epiglottis and glottis were found to be *somewhat* swollen, but not to any extent."

In the Museum of the Royal College of Surgeons in Ireland is only one specimen. In the catalogue it is thus described:^b—"The epiglottis and aryteno-epiglottidean folds are much thickened, and the opening consequently contracted." On examining the preparation, I find that, although the above changes are present, there is still plenty of room for respiration. The rima glottidis being very little, or not at all, affected. The swelling has principally taken place on the *back* of the arytenoid cartilages. In the Museum of Mercer's Hospital is also a single specimen. In it the mucous membrane is slightly swollen and wrinkled, but not by any means sufficient to close the rima. The latter specimens contrast remarkably with one of real oedema glottidis, in the Museum of the

^a Page 21, *l.c.*

^b Page 156.

College of Surgeons, taken from a man who died of suffocation, thus described :^a—"The aryteno-epiglottidean folds form two pellucid bladder-like elevations, the epiglottis is plump, the mucous membrane on its anterior surface being raised by serous effusion." They also contrast with a drawing of œdemea-glottidis in *Cruveilhier's Pathology*, LV., pl. 2, where the above-named folds are so much swollen as to come into direct contact in the mesial line, and thus completely closed the glottis.

It being thus proved that the glottis, although contracted by swelling, is not sufficiently so to be the cause of death by suffocation, where are we to seek for it? I believe in the lungs themselves; this will be evident, first, by examining those organs during life, when it will invariably be found that sonorous and sibilant râles are heard, even in the earliest stages. These symptoms are noted in all my own cases, and in many recorded by others. Dr. Croly observes, in one of his cases:—"No dulness on percussion, but râles are heard." But the chief evidence will be in the state of the lungs after death. Even in the earliest stage they are thus described^b by Dr. Bryant, in a case where death took place as early as three hours after the accident (no doubt a very rare occurrence):—"The lungs were found gorged with blood; trachia and bronchi congested and full of mucus; cervical veins and right side of heart full of blood."

At a later period after the accident, whether the operation has been performed or not, the state of the lungs is that of intense inflammation of various kinds. In a case noted by myself many years since, the trachea was red, both lungs were red, solid, scarcely at all crepitating on pressure, except towards the apex and along the anterior borders. In the cases reported in the *Times and Gazette*, *l. c.*, in which the lungs have been examined, in one there was acute pneumonia, in another, No. 10, right lung fixed by pleuritic bands and lower lobe hepatized. No. 11. Softening of right lung and surrounding inflammation round its root. No. 13. Trachea inflamed, and lungs congested. No. 14. Alone no evidence of bronchitis. In remaining cases the lungs were not examined.

In one case of Dr. Bryant's, "the mucous membrane was acutely inflamed, tubes filled with mucus, the lungs themselves also involved." In another case he says:—"Broncho-pneumonia

^a Catalogue pl. 156.

^b *l. c.*

appeared, proving fatal on the fourth day." In another case he found "the air tubes inflamed and filled with mucus; right lung completely consolidated, left one partially so."

Dr. Wilkes still more fully describes the *post mortem* appearances in (Case 49):—"The trachea and bronchi completely filled with tenacious purulent mucus, and some of them had also complete lymph casts; the mucous membrane highly vascular. Both lungs were more or less affected with lobular pneumonia, and on both sides was acute pleurisy."

In case 50 "the whole of the air tubes were inflamed and filled with secretion; there was pleurisy on the right side; a considerable portion of the lower lobes, as well as parts of the upper, were consolidated by diffuse inflammation." The same gentleman exhibited at the Pathological Society, April, 1860,* a specimen of the same accident:—"Acute inflammation of the whole of the air passages, combined with pneumonia; the under surface of the epiglottis, the interior of the larynx and trachea were covered with a false membrane, while the bronchial tubes were filled with purulent mucus." Thus I have proved that although the larynx is the part primarily affected, the child dies, not of the laryngeal obstruction but of the pulmonary disease; and in favour of this explanation I am happy to be able to adduce the valuable opinion of Dr. Geoghegan, who (at the meeting of the Surgical Society, reported in *Medical Press*, 2nd May, 1866) says "that Dr. Bevan had struck the right chord, that they had to deal with, not merely a case of œdema of the glottis, and obstruction from that cause, but with a general inflammation commencing in the lung, proceeding to the bronchial passages, and then to the smaller ramifications, ending ultimately in asphyxia."

How the lungs become so soon engaged may be a point of some difficulty to explain; it may be either that the child, in attempting to resist the swallowing of the water, some of the vapour heated to nearly 212° mixed with the atmospheric air passes directly into the trachea, and causes inflammation of the delicate lining membrane of the bronchi and air cells; or it may be that the sympathy is so great between the different parts of the respiratory organs in children, that as soon as the glottis is inflamed, the disease spreads with rapidity over the entire respiratory tract, a fact which every one conversant with disease of children will acknowledge to be very

* 12th May, 1860, *Medical Times and Gazette*.

likely. But no matter how the fact may be explained, if it can be distinctly proved that the child dies, not of the disease of the larynx, but of that of the lungs, it must be most illogical to apply our remedies to one of the symptoms, to the neglect of other and more important ones: instead, therefore, of removing a mere mechanical obstacle, which is only part of the disease, our attention should obviously be directed to the more dangerous and fatal one—the disease of the lungs. With this object in view, our main reliance must be placed on mercury, given so rapidly as to bring the child under its influence before the disease arrives at the stage when coma and congestion of the brain have set in. The treatment should somewhat vary according to the time which has elapsed since the accident; if seen in the earliest stage, before dyspnea has set in; the stomach, if full, should be unloaded by an emetic, and if the bowels have been constipated, a simple enema should be at once administered. By these means the action of mercury will be assured, and rendered more rapid. If the child is of full habit, or even of ordinary strength, I would apply a leech or two to the upper bone of the sternum; and as soon as the stomach had settled after the emetic, at once commence with the calomel, half-grain doses repeated every *half* hour. I would not wait till dyspnea had set in to commence this treatment, but would adopt it as soon as I had ascertained, by examining the epiglottis with my finger, that it had lost its natural elastic feel, and become enlarged and hard. Should the symptoms subside, it is easy to stop the treatment, or diminish its activity. If symptoms continue, or increase in severity, as is usual, then it is absolutely necessary to continue the mercury till either the salivation takes place or the well-known green stools are produced. In the meantime, the child's strength should be supported by good chicken-tea, or beef-tea, if it can be persuaded to swallow.

The temperature of the room should be kept up to about 60°; but instead of the bed being enclosed by curtains, free access of air should be permitted.

Stuping seems to relieve the pain of the throat, and is certainly of use in increasing the bleeding from the leech-bites, thus keeping up a moderate drain, and preventing the necessity for renewed application of them; but our chief reliance must be placed on the mercury. In every case I have seen the symptoms improve as soon as the green stools were produced. In some cases the patient appeared better a short time before that occurrence; but as a rule it

was immediately after it. In some cases the effect was instantaneous. In one case, on entering the hospital I enquired from the resident pupil how the patient was; he informed me that she was no better. On entering the ward I was surprised to find her sitting up in bed, breathing very much better, and apparently greatly relieved. Having questioned the nurse, she informed me that the resident had seen the child about a quarter of an hour previously, when she was apparently not better, but since he left the ward she had a copious green evacuation, and almost immediately she became vastly relieved. Dr. Geoghegan remarked the same rapid improvement in his case, and Dr. Croly reports:—"At twelve o'clock the child in heavy stupor; at half-past two bowels affected (green-coloured evacuation); at three o'clock breathing is in every respect better."

If the child was not seen till dyspnea had set in I would at once use the same treatment; but in addition I would rub his chest and axilla with mercurial ointment, and probably give the calomel in one or two grain doses every half hour, according to the age of the patient. The leeches should be repeated, if the child's strength will admit of it, but the effect should be most carefully watched before each leech is applied

Should the case not be seen till the child is semi-comatose, with congested lungs, and brain poisoned by venous blood, no doubt in some cases no treatment will be of use, but certainly tracheotomy can in no way either cure the inflammation of the lungs, or the congestion of the brain. The most efficacious treatment would be, if the child could swallow, to give the mercury, and use frictions over the entire body; to apply blisters over the chest, so as to produce, at least, redness of the skin, and over the surfaces to apply mercurial ointment; to apply heat to the extremities, and, if possible, to give stimulants in small and frequently repeated doses, as Dr. Croly did, in one of his cases, with success.

It may be said that a few cases have recovered after tracheotomy, and that (as I have remarked in my former paper) patients almost invariably appear better for some hours after the operation. In answer to these observations I would say, that they would have recovered more frequently without the operation, which is in itself one not unattended with danger, an opinion I have already expressed, and which has been fully borne out by cases cited by Dr. Wilkes in his already quoted memoir.* That the children appear

* *l. c.* sp., 132.

better after the operation is to be explained by the temporary removal of the congestion of the brain and lungs from the loss of blood, just as in other inflammations and fevers the loss of blood gives temporary relief, only to be followed by greater collapse and worse symptoms.

Two objections may be urged to the above treatment:—First, that leeching and tartar emetic have sometimes succeeded; and, secondly, that the mercurial treatment is either too slow, as Dr. Watson has urged, or too dangerous if given with sufficient rapidity.

To the first objection it may be answered that antimony very frequently fails. I have seen it used in very many cases. It was the treatment always adopted in Mercer's Hospital, and I have used it myself in all my own cases, and only resorted to tracheotomy after the antimony had failed. Besides, I look on antimony as a most dangerous medicine in young children. I have seen it produce most alarming symptoms; and in some cases, such as Dr. Croly's, the state of exhaustion of the patient would render it perfectly inadmissible. I never use it except to unload the stomach, and where that has been empty, I avoid it. I very much suspect that some surgeons who boast that they have had *extensive experience* in this class of cases, and that with them the antimonial practice has been completely successful, have mistaken many cases of mere scald in the mouth, which are *very common*, for the *more rare* and far more fatal accident, scald of the larynx.

To the objection, that the mercury will not act sufficiently early, I have only to adduce the above cases, which show that its effects can be produced in eight hours in some cases, and generally from eighteen to twenty-four hours; whilst children, even if left to themselves, live much longer after this accident. As to the danger of giving the mercury so rapidly—at first, I ventured with much doubt to give so much in so short a time, to young and delicate children; but having administered as much as forty-eight grains in twelve hours, without the slightest bad consequence, except a very moderate salivation in two cases, and a smart but by no means alarming catharsis in others, I have now no hesitation in using mercurial inunction, and calomel in from one to two grain doses every half hour according to the severity of the symptoms, and age of the patient. The leeching I use with more caution. I consider it of great value in strong children, by relieving the urgency of the dyspnea, and probably accelerating the absorption of the mercury.

The cases now published, with the kind permission of Drs.

Geoghegan and Ledwich, although I have only imperfect notes of them, were, when I saw them, fully as bad as any of the cases I had myself treated in former times by tracheotomy, with the usual fatal results. In fact, Dr. Ledwich's and my own cases, from their early age, were even more hopeless than ordinary; and in the latter case the difficulty of breathing came on with extreme rapidity, only two hours after the accident. I did not see Dr. Croly's cases, but from the description there can be no doubt of their severity.

DR. GEOGHEGAN'S *Case*.—A. H., on 9th March, 1865, was admitted into City of Dublin Hospital. *In company with Dr. Geoghegan, I saw her* at 8 o'clock p.m. She was a very stout, fat child, four years old; had swallowed boiling water out of a kettle, five hours previously. She was crying, but could not say where she felt sore. Dr. G. examined the throat, and stated that the tonsils were quite white. She is coughing, and keeps her hand to her throat.

Dr. G. ordered 1 gr. tartrate of antimony in 4 oz. water; a dessert spoonful to be given every ten minutes till vomiting produced.

An enema immediately, and as soon as the stomach settles after the vomiting; 1 gr. of calomel every half hour, till green stool produced. Two leeches to the sternum, and stuping over the throat.

On the following day Dr. Geoghegan kindly furnished me with a note, stating that extreme difficulty of breathing came on at about one in the morning, that the leeches bled freely. Copious green stools followed the administration of the calomel, and the symptoms gradually subsided.

On examining the fauces in the morning, a large grey slightly œdematous patch was seen still adherent; but the mother, finding that the child was relieved, took her home, not being able to remain away from her family.

DR. LEDWICH'S *Case*.—Fanny Smith, aged two years, a healthy child, attempted to drink boiling water from a kettle, at 10 p.m., on Saturday, 6th March, 1864. She remained well till one in the morning, when her breathing became difficult and stridulous. She was brought to Mercer's Hospital at a quarter before five, on the morning of the 7th, labouring under the usual symptoms.

I saw her at 9 a.m., with Dr. Ledwich. She had then stridulous breathing, pain in the throat, and rapid respiration. The epiglottis was large and hard. Dr. L. ordered one grain of calomel every half hour; leeches to sternum. He did not give an emetic, the child's stomach being empty. At half-past 6 p.m., bowels were opened, the stools being green, and a second time at 10 p.m. The breathing gradually became easy, and on the following day she was quite well, except some diarrhea and weakness. This case, of which I have only the above short notice, was fully as bad as any of the previous ones.

Under my own care.—Margaret Dowling, aged one year and eight months, admitted into Mercer's Hospital at half-past nine o'clock on Thursday, the 7th September, 1864, having attempted to drink water from the spout of a kettle just taken off the fire, at 5 p.m., she spat some out of her mouth. Her mother states that she remained well till about 7 o'clock in the evening. Dr. Ledwich saw her for me at 10 p.m. She was then labouring under intense dyspnea, with stridulous breathing. He ordered a leech to the sternum, and 1 gr. of calomel every hour. At 11 p.m. I saw her. She was breathing with great difficulty. Her face was nearly livid, and mucus was rattling in her throat. With great difficulty I introduced my finger and felt the epiglottis round and hard, but I could not get a good view of the fauces, as she closed her jaws spasmodically whenever I attempted to keep her mouth open. The leech had been applied, and has bled freely, and Mr. Samuels, the resident pupil (who attended the case with the greatest care), informed me that she is better since its application. If so, she certainly must have been very bad, for although she is dozing, her breathing is excessively difficult, almost gasping; mucus rhonchus over the greater part of the chest, but it sounds clear on percussion. I applied a stupe-cloth over the throat and leech-bites, and they began to bleed again, and I ordered another leech to be applied as soon as the bleeding ceased, should the infant's strength permit it; the calomel to be continued every half hour in grain doses.

8th.—At visiting hour, 9 a.m., the resident informed me that she remained in the same dozing state, with intense difficulty of breathing all night; too weak to admit of more leeching; but about six this morning she rather improved, and at 7 o'clock purging of green matter commenced. She has been better since. Her

breathing is still difficult, and the mucus is still rattling in her throat, but she is decidedly better. Her eye is clearer, and she is no longer in the dozing state in which she lay last night.

Whilst I was in the ward she had another copious green evacuation.

I ordered one grain of calomel, every fourth hour, and to continue stuping.

During the day the purging continued, and her breathing improved every hour.

9th.—The child is breathing quite well, and the mother insists on bringing her home.

She called at the hospital every day to inform me that the child had still some cough, but on the third day was quite well. Her mouth was not sore, and her bowels regular.

ART. XII.—*Case of Protracted Utero-Gestation; with Remarks.*

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I WISH to bring under notice the particulars of a case of uterogestation protracted for several weeks beyond the average period; and I have the less hesitation in doing so as the subject of it was under my constant observation, not only during pregnancy, but before its commencement, as well as for some time after its termination.

A lady, aged about thirty years, who had been pregnant six times, the pregnancy on two occasions ending in miscarriage, came under my care in November, 1863. She complained of frequent and often excessive menstruation, as well as of that form of neuralgia of the ovaries described by Dr. West as "ovarian pain," and by Dr. Churchill more fully as "ovarian irritation." She was also the subject of frequent hystero-epileptic fits. It would appear from the history she gave me that she had suffered for some years previously from functional derangement of the sexual organs, of which pain was a constant symptom, for which she had been subjected to a long course of local treatment, which included the repeated application

of caustics, and on one occasion of caustic potash to the os uteri. The uterine symptoms for which she consulted me were referred by her to a miscarriage in the fifth month of pregnancy, which occurred in the preceding April, about seven months before she came under my care. I found no evidence of any organic lesion, either of the uterus or of the ovaries.

This lady menstruated in December, 1863, the discharge appearing on the 28th, and ceasing on the 2nd January, 1864. About the middle of the latter month she suffered from troublesome nausea and vomiting, coming on every morning and subsiding towards evening. This morning sickness was of a very distressing and intractable character, and was only very slightly mitigated by medicine. Its characteristic nature, and its persistence made me attribute it to pregnancy, and to that alone, especially as it had been an early and troublesome accompaniment to all her former pregnancies; and my opinion was confirmed by the non-occurrence of menstruation at the expected period. This morning sickness continued almost without intermission, though occasionally slightly alleviated by medicine, from the 14th of January until the last week in March. She quickened on the 28th of March, when she first distinctly felt the fetal movements. Her pregnancy, evidenced by all the ordinary phenomena, proceeded favourably until the 12th of May, when she was threatened with miscarriage. She had severe uterine pains and some hemorrhage, which, however, yielded to treatment and to the enforcement of rest in the recumbent posture. Abdominal enlargement advanced in the usual manner until the 21st of August, when she had a severe hystero-epileptic fit, which lasted for seven or eight hours, during which she was almost completely unconscious. This fit was sufficiently alarming to cause some apprehension of an unfavourable termination, and was followed by a state of somnolence, which continued for about thirty-six hours. On recovering from this somnolent condition, she ceased to feel the movements of the child, for the first time since the 28th of March, nor was she again cognizant of them for some days. At this time I made a tactile and auscultatory examination of the abdomen, which I found to be as large as is usual at the seventh month of utero-gestation; but I could not detect any fetal movements. A well-marked uterine souffle or murmur was audible, but the pulsations of the fetal heart were not heard. This, of course, was not to be wondered at, as the lady was remarkably stout, and the consequent thickness of the abdominal parietes

might well prevent the fetal heart-sound being heard. On the evening of the 3rd October, having, as she believed, arrived at her full time, she was taken with labour pains, which increased in force till the following morning, when they abated, and disappeared during the day. The pains recurred on the evening of the 4th; increased during the night, declined towards morning, again disappearing on the 5th. This sequence of phenomena was repeated for four or five days successively. During an intermission I made a vaginal examination, and found the cervix uteri quite obliterated, and the os soft and patulous, so that, were it desirable, I could have introduced the point of my finger. Uterine pains, observing nearly the order and the duration described, continued to recur at uncertain intervals until the 21st of November, when I was hastily sent for near midnight. On arrival I found that labour had actually set in; the os uteri was dilated to the size of a shilling; parturition proceeded favourably, and she gave birth to a mature male child, at 5 o'clock, a.m., on the 22nd November, precisely seven weeks from the first appearance of labour pains. With regard to the child, the only circumstance at all noticeable was that it was born with a tolerably good head of hair, the lady observing that none of her former children had any hair whatever at birth.

For a week or ten days previous to the 3rd of October, there were present the usual indications of approaching labour, a sensible falling down of the gravid uterus towards the pelvis, and consequent relief from the distress caused by its pressure against the diaphragm, as well as slight apparent diminution of size in the abdomen itself. During the seven weeks which intervened between the first occurrence of uterine pains and the expulsion of the child there was an actual decrease of nearly two inches in the circumference of the abdomen, as ascertained by measurement.

I may here mention that on three separate occasions, subsequent to the occurrence of the spurious labour pains, there was a discharge of water from the vagina, each time to the amount of nearly, if not quite, a pint, according to the lady's own estimate. She thought the discharge was caused by rupture of the membranes, and each time expected labour to follow at once. This fluid, doubtless, accumulated in the space that sometimes exists between the amnion and chorion; for, though these membranes are generally in close apposition during the later months of utero-gestation, they are separated in the earlier months, the intervening space then containing a gelatinous fluid. We know, at all events, that in the later

months of pregnancy, fluid is sometimes found in this space, constituting a false amniotic fluid, which, when discharged at the beginning of labour, may be mistaken for the liquor amnii. Whether or not this was the source of the fluid discharged in the present instance is not of much moment as it has no obvious connexion with the protraction of pregnancy. It is, however, interesting as showing that a considerable quantity of fluid may accumulate external to the amnion. The discharge of the fluid was followed each time by relief from the sensation of fulness, and from the pain which she suffered.

Her convalescence was more favourable than after any of her former labours, all of which were attended with severe floodings, by which her life was more than once imperilled. So great was the loss of blood, and so difficult its arrest when last confined at the full time, in 1861, that she was advised by three medical gentlemen who attended her, never again to run the risk of pregnancy, as it would in all probability compromise her life. This advice she had ever in mind during her pregnancy, to the termination of which, she consequently looked forward with no small apprehension.

The period of utero-gestation in the case now related was, unquestionably, considerably protracted beyond the average limit of 275 or 280 days. It remains to fix as nearly as may be possible the limit of this protraction. The case supplies data sufficient to determine accurately the minimum duration of the pregnancy, and, within a day or two, its real duration. These data are:—

(a). The date of last menstruation, 28th December, 1863, to 2nd January, 1864.

(b). The occurrence of characteristic morning sickness in January.

(c). The perception of the fetal movements on the 28th of March, and their persistence.

(d). The threatened miscarriage in May, after the completion of the calculated fourth month of pregnancy, corresponding to the period at which she actually miscarried on two former occasions.

(e). The existence of a well-marked utero-placental murmur, with abdominal enlargement in August; and

(f). The occurrence of spurious labour pains in the beginning of October (marking, as I interpret them, the termination of the normal duration of gestation), when the cervix uteri was found obliterated. Any one of these signs taken by itself might be open to objection; but, in the aggregate, they afford evidence as strong as is ever likely to be adduced in proof of protracted gestation.

Besides these, however, my case furnishes another fact of importance in fixing the date of conception. My patient was, at the time I speak of, separated from her husband in order that she might be under my immediate care, being only visited by him occasionally. He was absent from the 26th of December to the 6th or 7th of January, when he returned and remained with her till the 10th. Before he again visited her the characteristic morning sickness had satisfied me that pregnancy had already commenced. This definitely fixes the 10th of January as the latest date on which conception could have taken place; so that the minimum duration of pregnancy must have been 317 days, or about six weeks more than the average.

It would be mere waste of time to discuss the value of the individual data on which the duration of pregnancy has been calculated. One of them, however, namely, quickening, perhaps requires a few remarks. As regards this sign there might be a possible mistake if it occurred in a primipara, but occurring as it did in a highly intelligent woman who had already experienced the sensation on six previous occasions, there is scarcely room for any doubt, and the less as the fetal movements continued to be felt, and with increasing distinctness afterwards. The unusually early period of pregnancy at which this sensation was felt deserves also to be noticed. The distinct perception of the movements of the child within eleven weeks and two days from conception is very uncommon. Denman, who is followed by most obstetric writers, says:—"Quickening happens at different periods of pregnancy, from the tenth to the twenty-fifth week, but most commonly about the sixteenth week after conception." Denman, however, gives no example, as far as I know, of its occurrence so early as the tenth week. Dr. Montgomery, of Dublin, the author of the most classic work on pregnancy, and of the ablest essay on its duration in our language, referring to Denman's opinion, writes:—"I confess that, with all my respect for Denman's authority, and appreciation of his sagacity and truthfulness, I felt for a long time sceptical as to the occurrence of quickening at such early periods; but in addition to the case above related, I met with another, more recently, at a still earlier period, so that I can no longer doubt." In this quotation Montgomery refers to two cases, related by himself, in which quickening occurred in eleven weeks and two days, and in ten weeks, respectively. He was probably in error in fixing the time of quickening in the latter case, for, finding that 213 days intervened between the perception of

the fetal movements and delivery, he deducted that interval from 280 days, the usual period of gestation, and the difference, 67 days, he considered as the time that elapsed between conception and quickening. To me it appears more probable that gestation was protracted than that quickening took place in the tenth week, for it is difficult to believe that the movements of the child can be distinct enough to be felt by the mother at so early a period. A similar mode of reckoning would make quickening to have occurred as early as the fortieth day in my case.

In cases of protracted gestation, where conception could not be traced with certainty to a single coitus, some physicians have been wont to deduct a certain number of days from the date of last menstruation in order to fix the commencement of pregnancy. Although conception is generally believed to take place soon after the cessation of the menstrual flux, yet it, undoubtedly, sometimes happens, and according to some obstetricians, often to be deferred till near the following period. As in any particular case it would be impossible to determine whether conception had immediately followed one menstruation, or merely preceded the one that is missed in consequence of its occurrence, those observers who have desired accuracy, chiefly, have deducted a period equal to a menstrual interval from the last menstruation, so as to fix the time of conception. For this purpose the distinguished Professor of Obstetrics in the University of Edinburgh, Sir J. Y. Simpson, allows a period of 23 days, and Dr. Murphy, late Professor of Midwifery in University College, London, has deducted 28 days from his cases; so that in the cases of protracted gestation recorded by these physicians, any error must be on the side of defect, and not of excess, as regards duration. In my case greater accuracy was obtainable without the necessity of having recourse to this plan; indeed, menstruation may be altogether excluded from the account. Its minimum duration was, as I have shown, 317 days, a duration which has few parallels in the annals of British medicine.

A very brief notice of those cases of pregnancy which are recorded as having attained a duration of 300 days, or more, may not be out of place. Dr. Merriman, who first in England investigated this subject, collected 114 cases in which gestation was reckoned from the last day on which the menses were seen, but excluding that day; in only four of these was gestation protracted beyond 300 days; in one it reached 309 days. Merriman's cases are, however, unsatisfactory, inasmuch as he made no allowance for

the probable interval between menstruation and conception. Dr Montgomery collected 56 cases, considered by him to be perfectly reliable, in which the date of fruitful intercourse was known, but in only one did the duration of pregnancy exceed 300 days, the husband having been with his wife on the nights of the 21st, 22nd, and 23rd of February, and labour occurring on the 21st of December; pregnancy lasted in this case, therefore, from 301 to 303 days. Dr. Reid, in his carefully collected series of 43 cases, in which gestation was counted from a single intercourse, gives only one in which it extended to 300 days. Three cases are recorded by Sir J. Y. Simpson in which gestation was protracted to 301, 309, and 313 days, respectively, after deducting 23 days from the date of last appearance of the menses. Amongst Dr. Murphy's cases two are found in which pregnancy is stated to have been protracted to 314 and 324 days, respectively, after deducting 28 days from date of last menstruation. Murphy's cases are, however, open to the objection that the subjects of them were hospital patients, on whose statements he was obliged to rely exclusively, and such statements are exposed to many sources of error, intentional as well as unintentional. The notes of the case which lasted 324 days were not taken by Murphy until six months after its termination. As he was satisfied with the data on which he calculated their duration, and as Dr. Taylor, in his *Manual of Medical Jurisprudence*, treats them as authentic, it is not for me to impugn their accuracy; their value would be much greater than it is had Murphy had the opportunity of observing them throughout. The late Prussian medical jurist, Dr. Casper, Professor of Forensic Medicine in the University of Berlin, commented upon them very severely, and blamed Taylor for attaching so much importance to them. More recently Dr. Murphy has met with another case in which he believes that pregnancy was protracted to 357 days, after deducting 28 days from the last menstruation. This case is recorded in the second edition of his *Lectures on the Principles and Practice of Midwifery*; the subject of it was a lady who had previously borne three children, two at the usual term, the third a fortnight later. The menses were last seen on the 10th of February, 1861; the motions of the child were felt about the middle of July; she expected her confinement in November, and felt her pains in that month, but the receipt of a letter gave her such a shock that the pains disappeared, and did not return till the 2nd of March, 1862, when she was delivered of a "feeble and diminutive" child. It appears that Murphy only saw

this lady once, when her pregnancy had already, as she thought, exceeded the average period, so that the value of the case is much impaired from this circumstance. Dr. Taylor, in the sixth edition of his *Manual*, refers to a case reported by Dr. Power, in his work on Human Pregnancy, in which gestation is said to have been protracted to 325 days. Taylor also mentions, in the same place, a case communicated to him by Mr. Chattaway, a former pupil. The healthy wife of a farmer, aged 36, menstruated for the last time in December, 1855, and quickened in the beginning of April, 1856; in the middle of September she was seized with severe false pains, accompanied with a discharge of mucus tinged with blood; and on the 19th of November was delivered of a female child of the average size. In this case pregnancy was apparently protracted, after deducting 28 days from last menstruation, to 330 days. Casper, referring to this case, says:—"It admits of another mode of computation: if we suppose that the last menstruation, 'in' December, occurred about the end of the month; and further, that the conception took place towards the end of January, then we have only a pregnancy protracted for about 300 days, which is nothing incredible. Moreover, nothing is said as to the usual menstrual cycle of this woman—nothing (so necessary for a critical estimation of the alleged perception of fetal movements at so early a date) as to whether the woman was a primipara. In what other science, may I ask, has there existed for centuries so total an omission of all critical inquiry as I have just proved to exist in forensic medicine, precisely the one of all others in which the phenomena observed require to be most sharply criticised!" Dr. Guy, Professor of Forensic Medicine in King's College, London, mentions a case (in the second edition of his *Principles of Forensic Medicine*) in which the minimum duration of pregnancy, as determined by the sudden death of the husband, was 308 days. The case was communicated to him by Mr. Hewitt, a former pupil of King's College, but no particulars of it are given.

The only other cases of pregnancy protracted beyond 300 days, which I have met with in the records of British practice, are those related by Mr. Robert Annan, of Kinross, in the *Edinburgh Medical Journal*, for February, 1857. In Mr. Annan's three cases he considers that gestation was protracted—in the first, for six weeks beyond the expected time; in the second, to 327 days; and in the third, to about the same period. These cases occurred, twenty, seventeen, and six years, respectively, before he related them, with

“some diffidence,” as “nearly as his notes and recollections permitted.” In Mr. Annan’s first case, the woman, considering herself at the end of the seventh month of her sixth pregnancy, had her nerves so strongly affected at a public mesmeric exhibition, as to be threatened with miscarriage. Instead of this, however, her confinement was postponed from the 5th of June, the day she expected it, to the 21st of November. In his second case, Mrs. —, aged 44, married in June, 1839; ceased menstruating in the end of December, and expected her confinement at the end of September or beginning of October, 1840. This event, however, did not take place till the 21st of November, when she was delivered, by the forceps, of a still-born female child. In his third case, Mrs. —, aged 26, menstruated on the 1st of April, 1836, and was delivered in February, 1837, of a male child, which weighed 10 lbs. 14 oz. Mr. Annan comments on this case, thus:—“Though the corroborating testimony of a mother who had borne live children at the full period was wanting, the defect was here amply made up by the male parent, a person of respectable status in society, who, on my expressing concurrence in the belief of the protracted period in this pregnancy, at once frankly stated that of this there could be no doubt whatever, as, during nearly the whole period above mentioned, he had not approached the nuptial bed.” “As far, then, as this case goes,” he proceeds, “it affords almost decisive evidence of protracted pregnancy; and, deducting a period of days for the interval between insemination and conception, the case seems as complete as the size of the child, and respectable human testimony, can make it.” Why, then, did Mr. Annan allow twenty years to elapse before recording it, and then only from memory! It is to be feared that if evidence more decisive than that furnished by his cases could not be adduced in favour of protracted pregnancy, the opinion expressed by Drs. Davis, Gooch, and Sir C. M. Clarke, in the Gardner Peerage Case, in 1825, would still be considered incontrovertible, viz., that the limits of gestation were fixed, and that protracted pregnancy was impossible. Yet these cases are referred to by Dr. Barnes, in the fortieth number of the *British and Foreign Medico-Chirurgical Review*, as illustrations of the vexed question of the duration of pregnancy, and without any expressed or implied doubt of their perfectly reliable character.

In Germany, Elsässer, “that most cautious enquirer,” as he is designated by Casper, who is apparently sceptical as to the existence of the quality of caution in British obstetric statisticians, in a paper

containing the particulars of 260 cases of pregnancy, entered in the *Journal of the Stuttgart Lying-in Hospital*, states, that in eleven of his cases gestation was protracted to periods varying from 300 to 318 days, reckoning from the last appearance of the catamenia.

In America, cases of protracted gestation have been frequently recorded. Professor Meigs, of Jefferson College, Philadelphia, in his work on *Obstetrics*, relates a case which he met with, in which pregnancy is alleged to have lasted 420 days, or nearly fourteen months, as ascertained chiefly from the statements of the woman herself, an hospital patient, and thus remarks upon it:—"Of course in relating this case, I do not consider myself responsible for the truth of the statements, further than they are worthy of confidence, in view of the character of the patient herself, and as the facts came under my notice. She had the appearance of perfect candour and sincerity in all that she said about it, and I have no doubt she thinks her pregnancy began in July, 1839, and ended on the 13th September, 1840." Besides his own case, Meigs relates one from Professor Asdrubali, in which a lady was affected with symptoms attributable to conception, on the 1st of March, 1796; and her husband died on the 22nd of the same month. On the 22nd of April, 1797, she was seized with symptoms of labour, and seven days afterwards gave birth to twins. This lady is stated to have had labour pains at the beginning of her ninth month, and to have discharged a large quantity of watery fluid from the uterus; these pains continued to recur for eight consecutive days. Dr. Meigs "cordially accepts the story of the accomplished author," and says that he finds in it nothing impossible to believe, more particularly as he has confidence in the case related by himself. Dr. Atlee, of Philadelphia, has recorded two cases in which gestation is alleged to have been protracted to twelve months, and to twelve months and seven days, respectively, and states that he "has not the least doubt of the truthfulness of the evidence in the cases." Dr. W. Stone, of Indiana, reported a case in the *Boston Journal*, for May, 1859, in which gestation is said to have been protracted to 330 days, reckoning from last intercourse. The subject of it was a married woman, who menstruated in February, 1858, and whose husband died on the 17th of March—the last intercourse having taken place on the 10th of the same month. She quickened on the 8th of July, and on the 3rd of February, 1859, gave birth to a female child, weighing 8lbs., whose osseous system was "extraordinarily developed." The mother had always sustained an

irreproachable character; and five physicians who investigated the case, were satisfied that it was one of protracted gestation. No wonder that Simpson should express his opinion that some of the cases of protracted gestation lately recorded, particularly "by our American brethren," exceeded the bounds of possibility. Such was also Murphy's estimate of them, until he met with the case in 1862, already mentioned, which appears to have put an end to his incredulity.

It is a circumstance deserving of attention, that in many of the recorded cases of protracted utero-gestation, signs of labour pains, &c., set in about the time of completion of the normal term of pregnancy. In several of the cases I have referred to, as well as in my own, this was observable. It was also noticed in an interesting case of protracted gestation, related by Dr. Tanner, in his work on *The Signs and Diseases of Pregnancy*, in which labour pains occurred on the 275th day from the last coitus, but ceased in a few hours, and did not return till the following day; again they ceased for twenty-four hours, and again returned; this happened daily for three weeks, when she gave birth to a female child, remarkable for its size, twenty-two days after the first occurrence of uterine pains, and 297 from the last intercourse. It was doubtless from observing this circumstance, that Dr. Jörg, of Leipzig, was led to believe that all cases of pregnancy exceeding 280 days, are merely instances of protracted labour. Jörg refuses to allow the period of gestation to exceed 280 days from the time of conception, and considers all cases of protracted gestation, as cases of "protracted parturition." The same idea would appear to have suggested itself to Denman, for he says:—"At the expiration of forty weeks the process of labour commenceth, unless it be hastened or *retarded* by some particular circumstance." Casper alone appears to have given to the occurrence of labour-pains, &c., at the normal end of pregnancy, its proper value as a proof of protracted gestation; furnishing, according to him, one of the three scientific points of any value in the diagnosis of disputed cases, the other two being the removal of all doubts as to the procreative capacity of the alleged father at the alleged time of conception, and the individual menstrual period of the mother.

A very unnecessary degree of importance has, I think, been attached to the degree of development presented by the child at birth, as probative of prolonged pregnancy. Dr. J. Matthews Duncan, for instance, says that the absence of extraordinary intra-uterine

development is a strong evidence against the reality of so-called cases of prolonged pregnancy. On the other hand, he considers its presence as "powerfully corroborative," of protraction in any individual case.

Dr. Duncan tells us that he has excluded two cases of supposed protracted gestation from the category of such cases, because the infants born were not at all larger than the former children of the same parents, who were born at the usual time. Such an opinion is at variance with nearly all experience. The over-maturity of the fetus is, according to Casper, a most variable idea, and only possessed of a negative value. If a child were "so very small that the mother did not know that her labour was over," it would be certain that it could not be the product of a protracted gestation. Doubtless there are various rates of intra-uterine growth, one fetus more slowly attaining maturity than another, just as one child attains manhood more slowly than another. In Casper's work on Forensic Medicine we find the details of the weights and measurements of 247 *mature* new-born children, which show that while the average weight of all the children was a fraction over seven pounds, there was a range of five pounds and a quarter, the maximum weight attained being ten pounds, and the minimum only four and a half; the measurements exhibited a range of six inches in the length. It is curious that the minimum weight as well as the minimum length was found in a boy. In a series of 391 children, born at the normal time, tabulated by Dr. Guy, in his book already referred to, it appears that the maximum weight was fourteen pounds, and the minimum, two pounds six ounces, while the mean weight was six pounds eight ounces, showing a range of eleven pounds and a half. Of 181 children, all of whom had reached the full time, as given by Dr. Guy, the average length was nineteen inches and a half, but there was a range of more than seven. In the obstetric institution in this city, of which I have charge, Julia Wale, a married woman, aged twenty years, recently gave birth to a well-developed child, fully as large as any fetus of eight months that I have seen, and yet, according to her account, it could not have been more than six months and a half, for this woman regularly menstruated till March, the catamenia appearing on the 10th, and ceasing on the 14th, of that month. Her child was born on the 2nd of October, 202 days after the last appearance of the menses. It only survived seven hours. Such facts as these show us what little dependence is to be placed on the degree of development of the child, either as favouring or opposing the belief in protracted gestation. Experience appears to

confirm what we would *à priori* expect, that it is for the sake of children whose *intra-uterine* development has been tardy that nature sometimes detains them in the womb until sufficiently matured to bear their extrusion from it with safety. Viewed in this light the occasional addition of a few weeks to the average duration of pregnancy appears not to be so very wonderful after all.

Nothing satisfactory, however, is known, or probably ever shall be, why utero-gestation is, in some rare cases, protracted beyond the natural, or, to speak more accurately, the average period, for the natural period has not yet been ascertained. Sir James Simpson has suggested the possibility of protracted mental agitation and anxiety producing this effect; and, in one of his cases, the lady whose pregnancy was protracted had endured much mental agitation owing to a steamer, in which she was a passenger, taking fire at sea; for seventeen hours she was exposed in an open boat, with but little clothing, and immersed in water. Mental anxiety was considered, by Mr. Annan, to have been the cause of the protraction in two of his cases. In my case, too, there was a good deal of psychical disturbance during the whole period of her pregnancy. But how many women suffer similarly, and yet protraction of that condition is not the result, premature labour being more commonly brought on by such influences.

The alleged importance of allowing for the time that may possibly elapse between insemination and conception, in calculating the duration of pregnancy, does not merit discussion. This interval was considered by Bischoff, reasoning by analogy from observations and experiments on some of the lower animals, to be about eight or ten days. Whatever physiological interest may attach to this question it possesses no practical or forensic importance. Pregnancy has been, and ever will be, reckoned from the last fruitful intercourse, when this can be ascertained with certainty. In the human female the length of time that may elapse between insemination and the fertilization of the ovum, is probably a variable one, and must ever remain a mystery, and, therefore, practically of no account.

In almost every class of mammals, in which accurate observations have been made for a sufficient length of time, the period of gestation has been found to fluctuate within limits as wide as those which have been observed in woman—either falling short of, or exceeding, the average duration. In the cow, the mammal most nearly approaching the human female in this respect, Baron Tessier found

it to vary from 260 to 321 days, showing a range of 61. Lord Spencer, whose observations were on a more extensive scale than Tessier's, found a still larger variation in the same animal, viz., from 220 to 313 days, the difference being as much as 93 days. Tessier found the duration of gestation in the mare, whose average period is believed to be about 330 days, to fluctuate between 290 and 419 days, the difference being 129; and these results have been confirmed by late observers. In the sheep, whose normal period is about 153 days, it has been found protracted to 157. In rabbits, whose period is 30 days, a protraction to 35 has been observed. In the sow, gestation has been found to vary in duration from 104 to 127 days. Similar variations are found in the period of incubation in birds, although the analogy between incubation and human gestation is less obvious than is that between the latter and gestation in the mammalia. Every one knows that a difference of three or even four days may exist in the time required by different eggs in the same clutch to bring the chicks to maturity. Although these facts have not been disputed there has yet been an unwillingness to admit the full force of the analogy borne by them to the protraction of human gestation. Dr. J. M. Duncan thinks this reasoning from analogy much over-stretched, and tells us that there are evident reasons for expecting that the period of gestation in woman should be limited on the side of protraction rather than in the lower animals. That he should so think is not surprising when he lays down the following dictum:—"That while absolute proof of the prolongation of real pregnancy beyond its usual limits is still deficient, yet that there is evidence to establish the probability that it may be protracted beyond such limits to the extent of three or even four weeks." These limits are, according to him, 275 days, minus the interval between insemination and conception.—*Vide Edinburgh Monthly Journal*, 1854.

Perhaps, after all, the ancients entertained a more correct opinion regarding the duration of gestation than modern physicians have done. Aristotle thought that there was good reason to believe that in the lower animals the period of gestation, generation, &c., were arranged in cycles, but that in mankind alone the period of gestation was subject to great irregularity. Pliny has given expression to a somewhat similar opinion, viz., that in all other animals there are stated seasons for reproduction, and periods of gestation; in man alone they are undetermined; gestation, he thought, lasted from seven to eleven months (lunar). To me it

certainly appears strange that the human female, exposed as she is to so many disturbing influences, psychical and moral, exciting and depressing, should alone be considered subject to a law of constancy in the period of gestation.

The case which I have now communicated is indebted for its chief value to the fact that I was cognizant of all the signs and symptoms of the gestation, so remarkably prolonged, *as they occurred*. This being so, I have thought it right to make it known, as there still continues to exist extensively among medical men an incredulity (unreasonable in my opinion) with regard to protracted gestation. The progress of medical science appears to be retarded as much by over-scepticism as it is by over-credulity. If it be true that utero-gestation may be in *rare instances* prolonged beyond its average duration, as my case goes a far way to prove, no harm can result from the admission of such an occasional occurrence. It appears to me to be the more necessary that every carefully observed case of protracted pregnancy should be placed on record, when we find Dr. Duncan propounding the dogma quoted above; and, more recently still, Dr. Tanner asserting that "all recorded experience of any real value, and all experiments upon animals, teach us that it is in the highest degree improbable that utero-gestation in the human subject has ever been prolonged beyond ten calendar months." Casper allows a greater latitude in the dogmata given by him, as sufficient for our guidance, as we would naturally expect, from his more extensive special knowledge and practical experience; these dogmata are:—"1. The usual duration of pregnancy is from 275 to 280 days. 2. Pregnancy may, however, indubitably be protracted beyond this, and that *even as far as the 300th day*. 3. Cases in which pregnancy is alleged to have been *protracted considerably beyond this, even to the eleventh, twelfth, and thirteenth month, have never been determined by accurate observations*; and allegations of this kind, in any individual case, are, therefore, completely *inadmissible*."

I shall conclude these remarks by quoting a passage from Sir Henry Holland's *Medical Notes and Reflections*, which appears to be peculiarly relevant to the subject:—"In questions of medical evidence there may be an excess of scepticism as well as of credulity. Sometimes this occurs in effect of a temperament of mind (not uncommon among thinking men) naturally disposed to see all things under doubt and distrust. There are other cases, where the same feeling, not originally present, grows upon the mind of

physicians who have been too deeply immersed in the details of practice. . . . From conscience as well as convenience, they come to confine themselves to what is safe, or absolutely necessary; and thus is engendered, by degrees, a distrust of all that lies beyond this limit."

NOTE.—This Paper was originally read at a meeting of the Bombay Medical and Physical Society, in November, 1865. A few verbal corrections and alterations have merely been made before transmitting it for publication.

ART. XIII.—*Flax Mills—their Machinery; Accidents Occurring Therein, with Suggestions for their Prevention.* By THOMAS H. BABINGTON, M.D.; T.C.D.; M.R.I.A.; Surgeon to County Londonderry Infirmary.

THE above may be considered an extraordinary text on which to ground a communication to a medical and surgical journal. The subject is one of importance. It, in my opinion, demands the attention of the public at large, as ratepayers, and of the Legislature; and I trust my brethren in the profession will concur with me in thinking that I am not out of place in asking them to consider the subject for a short space.

I have lived, during my professional life (commenced in 1835), in the flax-growing province of Ulster; first, in County Donegal, medical officer of a large dispensary district; afterwards at Port-stewart, County Derry; then, for thirteen years, medical officer of Coleraine Union Workhouse; and for twelve years surgeon to the County Londonderry Infirmary. In each year I have had under my care many cases of accidents from flax mills, of greater or less magnitude. In some cases one or two fingers have been lost; in others a whole hand, often a whole limb, sacrificed; and, occasionally, life lost by the frightful injuries inflicted.

Flax, when pulled from the earth, is steeped for a certain time, then spread on grass lands and dried in the sun; afterwards dried in kilns constructed for the purpose; then bruised, and carried to the mills ("*scutch mills*," as they were formerly called), there to be scutched or cleaned. And here begins the danger to lives and limbs.

In order to have the flax properly scutched or cleaned—that is, to have all extraneous substances removed, nothing to be left but

the fibrous texture to be afterwards spun and woven—it is first submitted to a process of bruising, or trituration, by passing it through grooved metal rollers, similar to the rollers of a threshing machine. When sufficiently crushed and softened, it is submitted to the action of the “fliers” or “wipers,” which are sharp knife-like boards, set upon a spindle, and driven, as are the rollers, with great rapidity, either by water power or steam. This last process separates the “shows,” as they are called, and leaves the flax clean.

In each of these parts of the manufacture of the staple commodity of Ulster accidents frequently occur; and in each succeeding flax season we can scarcely take up any newspaper published in Ulster, from September to March, without finding a record of a case or cases of accidents at flax mills, some attended with loss of life, others with the loss of an arm, fore-arm, or hand, many with the loss of fingers; all the sufferers more or less injured, maimed, or mutilated, and left comparatively useless for the rest of their lives, too often to be supported, with their young families, by the poor rates, or frequently having their own laudable exertions for self support aided and supplemented by their kind neighbours.

The most serious form of accident we meet with is occasioned by the rolling machinery. These are grooved cylinders, and frequently a portion of flax sticks in them, or goes in crooked. The machinery is working with great rapidity, and the attendant, who is “feeding” the rollers, eager and anxious, makes a snap at the flax to pluck it out. Either a finger, the cuff of a coat, the sleeve of a shirt, or a portion of a female’s dress (for women are constantly employed at these mills), is caught, the whole hand and arm is dragged or sucked in, and the whole limb smashed and mutilated before the machinery can be stopped. It has happened that the arm has been torn off from the shoulder!!!

The other form of accident is from the “fliers,” “wipers,” or scutching apparatus. Although these “wipers,” or knife-like boards, are sheathed, and work in sheaths, still if the workman, or “scutcher,” is not particularly attentive, he may place his hand, holding the “stricks” of flax, too close to, or rather in front of, the “wiper,” and will receive a most severe blow, sometimes severing a finger or two from his hand, always inflicting a severe compound fracture, extending through one, two, or three fingers or metacarpal bones.

From the very large number of cases which have come under my own personal observation I have selected twenty, the heads of

which I annex, as an illustration of the statements I have made, that these flax-mill accidents are attended with frightful consequences—maiming, mutilation, permanent disablement, and even loss of life.

No. 1. W. B., aged twenty-four.—Left arm comminuted, contused, lacerated, elbow joint opened, lower extremity of humerus fractured. The fingers were caught in the rollers in an attempt to disentangle a portion of flax, the whole arm was dragged into the machinery, and was frightfully crushed and lacerated. Amputation four inches below shoulder-joint. Recovered.

No. 2. J. M.—Ulcerated stump, following amputation of right arm for injuries of a similar nature.

No. 3. J. M. H., aged forty.—Right hand lacerated, fractured; wrist-joint laid open, ends of radius and ulna fractured. An attempt was made to save the hand; gangrene set in, amputation of fore-arm above the line of demarcation, on the seventh day after the accident. Tetanus set in on fifth day after the operation, and the twelfth from occurrence of the accident. Died on the fourteenth day from the commencement of tetanic symptoms.

No. 4. John Dinsmore.—Entangled in rollers, caught by sleeve of his coat, whole hand and arm dragged in, all the bones fractured, and muscles lacerated, except the deltoid; axillary cavity laid open. Amputation at shoulder-joint. Recovered.

No. 5. Mary M'Neill, aged forty.—Attending the rollers, her fingers were caught and dragged in, all the fingers of right hand were crushed off; the machinery was working slowly, and she dragged her hand out, and actually pulled the median nerve almost from the root; several inches of it were hanging out of the wound. Tetanus set in on the eighth day after the accident, and she died on the seventh day, fifteen days from the occurrence of the injury.

No. 6. J. M. G., aged fifty.—Compound fracture of three metacarpal bones from a blow of the "wiper," or scutching apparatus. Made a good recovery, but was under treatment from 15th January to 4th May.

No. 7. J. S., aged thirty.—Similar accident to No. 6. Good recovery, after three months' treatment.

No. 8. J. P., aged seventeen.—Lacerated wounds of fore-arm, no fracture. Recovery after two months' treatment.

No. 9. J. L., aged fifteen.—Compound and comminuted fracture of fore-arm and humerus, dragged into the rollers. Arm amputated. Recovery.

No. 10. G. W., aged forty.—Compound fracture of first and second fingers of right hand from a blow of the wipers. Recovery.

No. 11. Sarah M., aged twenty-six.—Caught in the rollers. Compound fracture of thumb and fore-finger of right hand; both amputated. Recovered.

No. 12. Pat. M'Fadden, aged twelve.—Compound fracture of three fingers; one amputated. Recovered.

No. 13. Hugh M'E., aged twenty-one.—Fractured fingers and lacerated hand. Recovered.

No. 14. J. G., aged twelve.—Lacerated and fractured fingers. Recovered.

No. 15. J. M'M., aged thirty-five. Fractured and lacerated hand; one finger amputated. Recovered.

No. 16. J. C., aged twenty-four.—Fractured fingers. Recovered.

No. 17. John C., aged thirty.—Compound and comminuted fracture of forearm, elbow-joint, and humerus crushed in the rollers; arm amputated. Recovered.

No. 18. James Corry, aged twenty-six — Similar injuries as No. 17; arm amputated. Recovered.

No. 19. Ellen Logue, aged eighteen.—Dress caught in the rollers; arm dragged in, crushed, fractured, lacerations, reached close to shoulder-joint; the head of the humerus was split; amputation at shoulder-joint. Died on fourth day.

No. 20. James D., aged seventy-six, a thin, spare, weakly man.—Whole arm crushed in the rollers; amputation close to shoulder-joint, three hours after. Died from exhaustion on the second day after the operation.

The foregoing selection of only twenty cases from the many I could enumerate is sufficient to arrest our attention. Two amputations at the shoulder-joint, five amputations of arm, two cases of tetanus following the injuries; and the remainder—fractures and lacerations of hands or fingers—frequently requiring partial amputations; and all giving rise to much suffering, and long, painful, anxious, surgical care, treatment, attendance, and supervision.

I have now, in conclusion, to offer a few brief suggestions in order to prevent these accidents, and for the protection of the workers at flax mills.

1. That all flax mills be registered.

2. That the machinery be so enclosed as to prevent the possibility of the fingers, hands, or clothes of the workers being caught or entangled therein.

3. That the mills and machinery be regularly inspected by some competent authority, to see that the machinery is properly enclosed and protected; and that if any owner of a flax mill be found working without being registered, and the machinery certified as being fitly and properly protected, that he be subject to heavy penalties, and liable to pecuniary damages, should any accident occur from his neglect.

4. That all scutchers be paid proper daily wages, and not by the quantity cleaned, as by the cwt. or stone, as their eagerness to earn large wages makes them reckless and hastily careless.

5. That no public-house for the sale of spirits be allowed to exist within a mile of a flax mill, and that a severe penalty be imposed on any person bringing intoxicating liquors to a flax mill. Intemperance and drinking are most frequent about these mills, and lead to carelessness, indifference, and accidents.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Contributions to Medicine and Midwifery. By THOMAS EDWARD BEATTY, M.D.; M.R.I.A.; M.D., T.C.D., Hon. Causa. Dublin: Fannin & Co.

THIS volume is a collection of the principal papers contributed by Dr. Beatty to this Journal, with some others that were published in the pages of our contemporary, *The Dublin Medical Press*, and some more elaborate essays written for *The Cyclopædia of Practical Medicine*. These papers illustrate the history of some of the most important questions in medicine, and serve to show the means by which their author has arrived at the eminence he has attained, an eminence that has enabled him to say in his preface, with no less truth than justifiable pride, that it cannot be laid to his charge that in republishing these contributions he has adopted this method of bringing himself before his professional brethren, with a view to future reward or promotion; for their unbounded favours have left him nothing more to wish for, or to which he could aspire. "The many evidences of the esteem of my brethren in both branches of our profession, with which I have been honoured, might," he says, "well satisfy the most soaring ambition, and afford grounds for the deepest thankfulness, which I now desire to express; and again I repeat that my cup of professional honours is full, even to overflowing."

Success, such as is here pictured, and evidenced not alone by the public demand for his services, but by the honours heaped on him by his professional brethren, stimulates an inquirer to ask—on what does it depend? In this volume we have the answer. Here we have indisputable proofs of zeal, of untiring industry, of ability, and of that power of observation and reflection which can alone make experience valuable. When to these we add the happy personal and bright social qualities that in so remarkable a degree characterize the author of these "Contributions," we at once see how it was that his brilliant success has been obtained. But important as social and

personal character may be, it is only where they serve to adorn a life of earnest real labour, that they ensure success; and valuable as these essays are in a scientific point of view, they are, we conceive, still more so as indicating "footprints" that should stimulate us "to act that each to-morrow may find us further than to-day"—and as showing that if we would emulate the success of their distinguished author, we must, like him, be "still achieving, still pursuing."

The volume contains twenty-three chapters, the first and second of which consist of papers by Dr. Beatty's father, the late Dr. John Beatty. One "On the use of Instruments in Cases of Difficult Labour," and the other "On a Species of Premature Labour to which Pregnant Women are Not Unfrequently Liable." This latter is the paper in which Dr. Beatty established the doctrine now universally held as to the influence of constitutional syphilis in the parents in causing the death of the fetus in utero. The chapter on the use of instruments is a paper read by Dr. John Beatty before the College of Physicians, in 1829, at a time when the leaders of the Dublin school of midwifery had almost entirely banished from practice the use of the midwifery forceps. The subject so ably opened by his father is advocated by Dr. Beatty himself in the two following chapters—one being a "Report of the South-eastern Lying-in Hospital," an institution established by himself; and the other a paper originally read before the Dublin Obstetrical Society, and published in the twenty-first volume of the first series of this Journal, entitled "Cases Illustrative of the Use of the Forceps." To these papers the Dublin school is mainly indebted for the great revolution it has undergone with reference to the use of the forceps. The extent of this revolution may be judged of, when we state that when Dr. Beatty read the account of his cases illustrating the use of the forceps at the Obstetrical Society, the gentleman who occupied the chair, who had been master of the Lying-in Hospital, and who, for forty years enjoyed one of the largest practices in this city, declared he had *once tried* the forceps and failed; and that during Dr. Clarke's mastership of the same hospital the forceps was used but once in 728 cases; whereas, it appears from the last published report of the hospital that Dr. Shekleton used it once in every 67 cases, and we have reason to believe that it is now used still more frequently with the effect of diminishing, in a most gratifying manner, both the maternal and infantile mortality of the hospital. Dr. Beatty may well feel proud of the part he bore

in this movement, and both his own papers and those of his father are worthy of most careful study as models of argument, and as illustrating the history of the Dublin school in reference to the forceps.

Chapters V. and VI. treat of the means of preventing uterine hemorrhage after delivery, of the influence of ergot of rye on the fetus in utero, and of the use of chloroform. From an early period Dr. Beatty has advocated the use of ergot of rye as a means of stimulating uterine action, of preventing hemorrhage, and of neutralizing the retarding influence produced by chloroform on the uterine contractions. With regard to the injurious effect of ergot on the fetus, Dr. Beatty attributes it chiefly to a poisonous influence consequent on absorption of the medicine, which he is led to believe does not take place under two hours. Our own experience does not allow of our adopting this view; as, though it may be true, as Dr. Beatty argues, that after a certain length of time ergot may produce a poisonous influence on the child; we believe the greater danger arises from the obstruction to the circulation through the placenta caused by the almost continuous uterine contraction it frequently produces. For this reason we do not ourselves use the ergot to stimulate a torpid uterus, as we think the forceps a much safer means of expediting delivery; and as the conditions that should be present to justify the use of ergot will permit of the safe use of the forceps, we prefer this instrument. But it is chiefly as means of preventing uterine hemorrhage that Dr. Beatty dwells on the use of ergot in these chapters; for this purpose he administers the ergot either after the head has been expelled, or when it is just about to be so. At this period the child cannot suffer any injury, and the influence of the ergot is most beneficial. With regard to chloroform, Dr. Beatty is a warm advocate for its use. For administering it he employs an inhaler, made by Coxeter, by means of which the dose and the degree of its dilution by atmospheric air can be regulated. He makes the patient hold the instrument in her own hand, so that as insensibility approaches the muscles relax, and allow the hand holding the inhaler to fall away from the mouth and put a stop to the administration of the drug. As we have already intimated, Dr. Beatty often gives a dose of ergot before beginning with the chloroform. We have no doubt many of our readers are familiar with the following description of a chloroform case; but even so, they will, we are sure, be glad to meet such a graphic description again:—

“The course of a labour treated with chloroform is usually as follows. Take, for instance, a first case, in which labour has been going on for eight, ten, or twelve hours, with that increasing amount of suffering which all females experience until the os uteri is nearly dilated. At this time we know that the pains usually become most severe; and the patients lose their self-control, becoming uneasy, impatient, and loudly complaining of the sufferings which they undergo. If, under these circumstances, we now commence the judicious use of chloroform, the first effect, after a few inhalations, will be calmness. Instead of tossing about in the bed, the patient becomes perfectly quiet, she experiences relief, and the friends who are standing around begin to be astonished at the almost instantaneous effect which has been produced. They had become anxious at the amount of suffering she seemed to be undergoing, and are now surprised at the calmness which has been brought about through the assistance of chloroform. I would say that in eight cases out of ten the patients express the most exalted kind of satisfaction at the relief thus obtained. They sometimes say, “Oh! this is so delightful, so pleasant!” In fact, a feeling of inexpressible happiness is produced in most patients by inhaling the chloroform, for perhaps two or three minutes, in the way I will presently mention. I have seen a woman who had been tossing about in her bed, perfectly uncontrollable (because to a certain extent delirious, as such patients often are) lie calmly and quietly down, without any stage of excitement, and remain so for the space of four or five hours, without for one instant losing her consciousness or self-control, or making loud exclamations, or removing herself from the position in which she had been directed to remain.

“The next effect which is produced by the judicious administration of chloroform is the total disappearance of that horrible aching in the back, of which patients complain in the intervals of the pains, and which makes them call on the nurse in attendance not to relax the pressure on the back which it is usual on such occasions to make. This intolerable ache is never felt after the administration of chloroform has begun; so long as it is kept up, the distress is done away with. After some time the patient will call out loudly for more chloroform, feeling the benefit which she has derived from its previous administration. We accordingly let her have a little more, and by degrees, as she becomes conscious of the approach of the pains, she takes the medicine again, and in this way passes hours without suffering. Some patients will sleep in the intervals of the pains. We know how valuable such natural rest is during labour; and I have observed that the administration of chloroform does not in the least degree interfere with it; on the contrary, it rather helps to produce natural sleep, when not pressed to the extent of sopor or insensibility. As the time of delivery approaches, the amount of chloroform should be rather increased, because the patients suffer more at this

time than at any other previous stage of their labour; and it will require a larger quantity for the purpose of alleviating pain, though not enough to produce the full effect which is produced in surgical practice. In fact, I can testify most distinctly that in no case will it be necessary to produce sopor or stertorous breathing in the course of the labour; and it is worthy of remark that the patients, when the labour is over, will tell you that they knew they had the pains, but did not feel them.

Chapter VII. is on purulent effusions into the joints, &c., in puerperal women; and in Chapter VIII. we have an account of a most remarkable case of pregnancy, complicated with a tumour occupying nearly the entire pelvis. In this case it was feared delivery could only be accomplished by the Cesarean section; but as labour set in the tumour was gradually pushed aside so as to allow of the child, which presented with the breech, being born *per vias naturales*; and the patient made a good recovery, from which Dr. Beatty says, "we may draw a useful conclusion with respect to the danger of deciding too quickly upon the impossibility of delivery being accomplished through the natural passages, and the necessity for performing Cesarean section."

Chapter IX., which is illustrated by a most beautiful coloured drawing is on a rare and hitherto unnoticed form of death in cancer of the uterus. In cases of cancer of the uterus the ordinary causes of death are, Dr. Beatty says, irritative fever, hemorrhage, exhaustion, or peritonitis; but in three instances that came under his notice death occurred in another form, viz., by uremia, in consequence of the cancerous deposit extending to the bladder and blocking up the ureters, so as to prevent the escape of urine; this causes the ureters and pelves of the kidneys to be enormously over-distended, till at length no further secretion can take place, and the blood becoming poisoned, the patient dies comatose and convulsed.

Chapter X. is on cases of retroflexion of the uterus successfully treated. Chapter XI. is on a remarkable case of calculus in the female bladder; and Chapter XII. on inversion of the urinary bladder through the urethra with large prolapsus of the rectum in a female child. We regret that the space at our disposal will not permit of our noticing these essays at length; but we pass them the more readily as it is not very long since they appeared in full in our pages.

Chapters XII. and XIV. we associate, as showing that, important as Dr. Beatty's communications have been in reference to midwifery, he has been an attentive student of all the phenomena of disease, and not a mere specialist. In the first of these chapters we have a brief record of some cases in which a *frottement* was observed in inflammation of the peritoneum, a subject afterwards taken up and dwelt upon by Dr. Bright; and in Chapter XIV. we have an account of the case of aneurism of the abdominal aorta, with the dissection and observations, first published in 1830, and from the publication of which, as Dr. Stokes has stated, in his book on *Diseases of the Heart and Aorta*—"our knowledge of the diagnosis of this disease may be safely said to date."

The four chapters that follow are reprinted from the *Cyclopædia of Practical Medicine*, and like those we have just noticed, show the extensive range of subjects embraced in Dr. Beatty's contributions. They are on impotence, rape, doubtful sex, and persons found dead.

We have next a chapter on the operation for the cure of dysmenorrhea—"A mode of treatment that (Dr. Beatty thinks) has not been as fully appreciated as its merits demand." In performing the operation he still uses the large three-bladed speculum, and Sir James Simpson's one-bladed hysterotome.

The following chapter treats of "Plastic Operations on the Female Genito-Urinary Organs," and describes operations performed in cases of vesico-vaginal fistula, and of prolapse of the uterus; and then the volume closes with three addresses, two of them delivered at the opening of the seventh and twenty-fifth sessions of the Dublin Obstetrical Society, and the other at the opening of the Winter session at the City of Dublin Hospital, on the 6th of November, 1865.

The original publication of the papers we have thus briefly noticed took place at intervals from the year 1830, when the memoir on aneurism of the aorta first appeared, till the present year. They have done good service in their day, and in their present form will do still more; and we are sure our readers will join with us in the sincere wish that our pages may long continue to be enriched by occasional contributions from the pen of their distinguished author.

RECENT WORKS ON CHOLERA.

1. *The Antidotal Treatment of the Epidemic Cholera.* By JOHN PARKIN, M.D. Third Edition. 8vo. Pp. 321 London: John Churchill and Sons. 1866.
2. *The Nature of Cholera, as a Guide to Treatment.* By WILLIAM SEDGWICK, M.R.C.S., &c. New Issue. Post 8vo. Pp. 200. London: Walton and Maberly. 1866.
3. *On Cholera, its Nature and Treatment; being the Debate in the Harveian Medical Society of London.* Edited by Dr. C. DRYSDALE. Pp. 34. London: Robert Hardwicke. 1866.
4. *Cholera; A New Theory.* By C. DUDLEY KINGSFORD, M.D. Pp. 16. London: John Churchill and Sons. 1866.
5. *Brief Remarks on Cholera.* By ROBERT J. SPITTA, M.D., Lond. Pp. 15. London: John Churchill and Sons. 1866.
6. *On the Treatment of Asiatic Cholera.* By ARCHIBALD BILLING, M.D., A.M., F.R.S. New Edition, Revised. Pp. 14. London: John Churchill and Sons. 1866.
7. *Mechanical Treatment of Cholera.* By A PHYSICIAN. Pp. 11. London: John Churchill and Sons. 1866.
8. *The Human Blight and Cattle Blight.* Pp. 30. London: Longmans, Green, and Co. 1866.
9. *On Epidemic Diarrhœa and Cholera, their Nature and Treatment.* By GEORGE JOHNSON, M.D., Lond. Pp. 32. London: Robert Hardwicke. 1866.
10. *The Arrest and Prevention of Cholera.* By ARTHUR ERNEST SANSOM, M.B., Lond. Pp. 131. London: John Churchill and Sons. 1866.

IN the last number of this Journal we reviewed some works on cholera, and drew attention to those facts in connexion with the progress of previous epidemics which we considered of practical importance. Then we were only threatened by the disease, now it has established itself in our city; we have reason to believe that

reports of the utmost value, at least as regards the elucidation of the laws in accordance with which cholera is diffused, will, at the termination of the visitation, be issued under the direction of the official guardians of public health; and we also look forward to the publication of reports on the clinical and pathological features of the disease by some of our hospital physicians, who have carefully watched the symptoms and morbid appearances; till we are in possession of these we think it better to say little of the present outburst. A few things, however, confirmatory of our previously expressed conclusions may be briefly alluded to. The circumstances under which the disease appeared in Dublin were such as to produce in most minds, we think, a clear conviction that to human intercourse is due its transmission from city to city; moreover the frequency with which several members of a family were attacked when living in filthy and ill-ventilated rooms, taken in connexion with the small number of nurses and other hospital servants who have suffered, shows that whatever the specific poison may be it is very largely dependent for its vigorous propagation on such favouring circumstances as want of cleanliness in habitation and person, overcrowding, and contamination of the air with fecal matters. In London, according to the reports of several medical officers, districts which in previous epidemics afforded a frightful mortality, but which have since undergone improvement in ventilation and sewerage, have either altogether escaped or had but few cases; when the epidemic commenced there, it was co-extensive with a certain water supply, to a degree which justified the suspicion that if the actual germs of the disease were not conveyed in the water, at least its impurity must have produced in the systems of those who drank it a greatly increased tendency to receive and develop them; we therefore repeat our advice as to fresh air, the boiling and filtering of water through animal charcoal, the flushing of drains, the use of disinfectants, and the washing of the hands and mouths of those who are in actual contact with the sick, before they eat; but if such precautions are taken we do not think that those about the sick are exposed to any particular risk, if they be at the time in good health themselves; this we think is important; among the poor living in places in the last degree unfavourable to health, and perhaps debilitated by want of food, and sometimes by excesses, the onset of the disease has frequently been sudden; but in all the cases in private, of which we have ourselves heard, a neglected diarrhea had gone on, in some cases for days, before the characteristic choleraic

member of our own profession has, victims; but it was clearly ascertainment on any case of the disease. Atmospheric condition may be the still, we understand, entertained experience of this outburst is not the wind was high, and from all continuous, and the temperature the earlier weeks; on the other absence of calm still air, with mists, e, in increasing the rapidity of the re. On all such matters, however, site information from the reliable 1. Regarding the treatment of the urge the opinion expressed in our l to which, on the faith of some urnal, we drew attention, received on, who had charge of the cholera ie impression left on his mind was the disease, and after trial by an ished we would hardly suggest its

ny new works have appeared, some show that their authors are very so difficult a subject—but still, a erving of attentive perusal.

coal and carbonic acid are direct in which causes the symptoms of grains of the bicarbonate of soda ssful of water, in which a dessert rred, the more effectually by its o be made effervescent by twenty lissolved in half a wineglassful of mediately; or, if more agreeable to given separately; when the collapse mbines with the soda carbonate of come on very suddenly, ether and may be tried. Sometimes, too, in e introduction of hot air under the condary fever and suppression of all frequently-repeated doses of

turpentine useful, in addition to the effervescing draughts; the interval between the doses should vary, according to the urgency of the symptoms, from fifteen minutes to two hours, but in no case of pronounced cholera does he allow a greater interval than half an hour to elapse between the doses; he is of opinion that all other measures, even frictions, are useless, and often injurious. Not only does he consider carbonic acid curative, but he maintains that the use, twice daily, of such draughts, or of a teaspoonful of freshly-prepared charcoal, beaten up with an egg, wards off cholera; he does not, however, claim much efficacy for the treatment unless commenced either before or at the beginning of collapse. He supports his views by two kinds of evidence; he endeavours to prove that the success which attended the saline treatment of Dr. Stephens and the calomel treatment of Dr. Ayre in the hands of some physicians, and the want of success which resulted from them in the practice of others, find their explanation in the circumstance that in the successful cases the patients were allowed to have soda water or effervescing draughts at the same time; and he gives us the statistics of the treatment of cholera by his plan in Spain, in 1834, and in Jamaica in 1851-52, together with those of some trials made at St. Bartholomew's Hospital in 1849; according to the Jamaica table the deaths amounted to little more than eleven per cent. If we did not know how often conclusions which had received the support of statistical evidence have been found erroneous, we would be compelled to say that Dr. Parkin has proved the superiority of his treatment; but there are so many ways by which fallacy may creep into statistics that we would value more highly the impression left upon the mind of a single accurate and unbiassed observer who has seen and weighed all the circumstances, than any conclusions merely supported by figures. That Dr. Parkin has been most zealous and conscientious in his investigations we are convinced—that he must firmly believe in a theory for the proving of which he has travelled so far and toiled so laboriously, we are sure—but that he has approached the experimental evidence with a mind unwarped by foregone conclusions, we doubt. The inhabitants of Herefordshire enjoyed a singular immunity from cholera; the men who worked in the Broad-street Brewery during the appalling outbreak around it escaped, because, says Dr. Parkin, both were constantly enjoying the advantages of carbonic acid, the former from their cider and the latter from their beer; but these very examples have been often, and we think more plausibly, quoted to prove not the

prophylactic efficacy of carbonic acid, but the advantage of abstaining from drinking water of doubtful purity during cholera times. Dr. Parkin's views have been before the profession for more than thirty years; and it is to be feared that their adoption in other hands could not have been attended with such signal benefit, or we should have a greater body of British evidence in their favour; nevertheless, seeing how little impression other measures make upon the disease, we feel justified in hoping that they will receive a renewed trial, especially as the effervescing drinks are at least grateful to the patients, and seem good against the vomiting.

We have carefully read Dr. Parkin's book from cover to cover; for if anyone has really anything worth saying about this disease we wish to be able to draw our readers' attention to it; may we venture to suggest to him and to others who write on cholera the importance of being brief; whether a man's views are worth bringing before the public or not must, of course, be left to the sobriety of his own judgment; occasionally, when the very principles by which we should be guided have to be discussed, as was done in Dr. Johnson's work, it may be necessary to show where the reasoning of previous authors was false; even then it should be done, as he did it, briefly; but in other cases, when an author thinks it relevant to the question to demonstrate the inutility of methods of treatment different to his own, he may surely, having proved this as a fact, be content with indicating, in the fewest possible words, his ideas of the fallacy of the principles on which they have been recommended. If any one can show us a mortality of ten, twenty, or even thirty per cent. as the result of his treatment of undoubted cases of cholera at the height of the epidemic, we will listen to him with respect though he do not prepare us by proving both how few actually did recover under other treatments, and how impossible it was that it could be otherwise.

Mr. Sedgwick believes that a morbid impression is made upon the sympathetic system *through the stomach*, or at least through it and the upper portion of the intestinal canal. He calls attention to the similarity that exists between the symptoms in cholera and, in conditions which admit of being directly referred to an affection of the sympathetic nervous system produced through the medium of the stomach, such as the shock which follows blows on the epigastrium, or perforation by an ulcer; he then proceeds to analyze four of the principal phenomena of cholera, namely—the suppression of urine, the altered condition of the blood, the flux from the stomach

and bowels, and the failure of the circulation, with the view of showing that these symptoms find a more satisfactory explanation in his theory than in any other. He does not state of what nature he conceives the agent to be which thus impresses the sympathetic, but he considers there is no evidence of the presence of a poison in the blood. The arguments by which he supports his views would not bear condensation, and any examination of them would require at least as much space as they themselves occupy. We recommend all who wish to have a philosophic view of cholera to read Mr. Sedgwick's book; and we strongly urge all who feel tempted to rush into print with some brilliant theory as to its nature, to do the same; they will see that before they are in a position to do so, they should not only have seen the disease, and have ascertained the conditions under which it has appeared in other countries, but they should be as thoroughly acquainted, as in the present state of science is possible, with the functions of the nervous system, with the forces which maintain the circulation of the blood, and with the endowments of the blood itself. There is one characteristic of Mr. Sedgwick's book which we commend to the imitation of all authors, the scrupulous care with which he acknowledges his obligations to those who have preceded him in the same field of inquiry. In the last number of this Journal we ventured to draw back attention to venesection as a measure capable of favourably influencing the course of cholera; it is the remedy on which Mr. Sedgwick would principally rely; he shows the importance which was attached to it in the earlier epidemics, and then sums up his own experience and views regarding it. After narrating a case which was benefited, though not saved, by it, he says:—

“Since the occurrence of this case, which was valuable as a practical illustration of the correctness of the theory that bleeding in cholera is antagonistic to collapse, I have had many opportunities for observing its effect on the disease, and my experience is that in all cases where it can be employed before the collapse has assumed a hopeless character, it is the most important agent we possess, not only for arresting the more urgent symptoms of cholera, but as a necessary introduction to the use of remedies which would be inert without it.

“Bleeding may be considered to have a twofold influence for good over the disease, which is exhibited, first, in its general effect on the circulation, and secondly, in its special effect on individual symptoms. Respecting the first of these effects, it is evident, from the authorities which have been cited in favour of bleeding, that the most marked results

following its successful employment depend on restoration of the circulation. This important fact is fully substantiated by all who have had opportunities for observing the effect of bleeding on the disease; and it is chiefly due to the unloading of the venous system, and the comparative diminution of the red particles of the blood; in consequence of which absorption is quickened, if not even in some cases restored, where such power has been previously suspended or lost. This is the primary indication to be fulfilled in the treatment of the disease; for without the power of absorption, the loss of fluid by the flux from the alimentary canal cannot be supplied by the amount of liquid taken into the stomach, and even medicines in such states of the system are inert; whilst the artificial introduction of saline mixtures into the veins shews that although the physical state of the blood is a great cause of the peculiar cholera symptoms, it is only so secondarily, and that unless the power of absorption is at the same time restored, it is useless to expect that the results obtained by saline injections will be permanent. Hence also it may be urged that whilst the physical change in the condition of the blood is closely connected with cholera collapse, it is probably due quite as much to an excess of the red particles, as it is to a diminution of the watery constituent. Bleeding might therefore be theoretically inferred to be necessary in cholera to produce (1) a diminution of red particles, and (2) an increase of the watery part of the blood. The combined result being that in place of a progressively fatal thickening of the blood, which kills by stoppage of the circulation—either primarily, as in death during collapse, or secondarily, as in death during the consecutive fever, which follows the prolonged arrest of the blood supplied to the alimentary canal—there is substituted a fluid which admits of the organic functions being, for a time at least, restored; for the blood, in becoming more capable of propulsion by a partial abstraction of its red particles, is at the same time rendered more capable of absorption, and by consequent self-dilution more fitted for the maintenance of organic life.”

After bleeding in severe cases, and without it in milder ones, he recommends turpentine. He says:—

“The subjoined mixture, occasionally combined with small doses of chloric æther, is that which I have been in the habit of giving in nearly all stages of the disease:

“R. Olei Terebinth: ʒss
Liquoris Potassæ ʒj
Muc. Acaciæ ʒss
Aquæ Carui ʒvss

“Two tablespoonfuls to be taken every ten or fifteen minutes for the first three or four doses, and subsequently less often.”

In Dr. Johnson's pamphlet the indiscriminate administration of castor oil in epidemic diarrhea is by no means recommended; he points out the impropriety of opiates and astringents while we have reason to think morbid secretions still lie within the bowels; once these have been removed, either by abundant natural motions or by medicine, he gives five or ten drops of laudanum. During the collapse of true cholera, he recommends castor oil only where there is "over-distention of the bowel by liquid, and accumulation and retention of offensive viscid semi-solid secretions." He, too, recommends venesection; where there is rapid breathing, with a feeling of impending suffocation, and a cessation of vomiting and purging; he believes that it affords the only hope of saving life. We do not think that many of the Dublin physicians are actual supporters of the eliminative method; yet we believe the attention which Dr. Johnson drew to it has influenced the treatment of this epidemic; opiates have been given more sparingly, and castor oil, with or without turpentine, has been thought advisable where consecutive fever, with head symptoms, has been united with cessation of purging and suppression of urine. Dr. Johnson lays much stress on the general management of the patient by rest in bed, and the other means to which, in the last number of this Journal, we drew attention.

Dr. Kingsford believes cholera is caused by phosphorus, probably in the form of phosphuretted hydrogen, which in cholera seasons is not neutralized by ozone. As a matter of experience he has found large doses of calomel useful, and this he explains on the supposition that it acts as a chemical antidote.

Cholera, Dr. Billing very dogmatically tells us, is a species of fever, and the remedy is this:—

“Water, half a pint.

Tartar emetic, 2 grains.

Sulphate of magnesia, half an ounce. Mixed.

The dose is, for an adult (from fifteen years upwards), a tablespoonful every half-hour; for a child of a year and a half or two years, a teaspoonful; and for the intermediate years a proportionate dose.”

The mechanical treatment of cholera consists in laying the patient on his back on a board, and giving him a rocking movement. We can only say, after the author has tried it, we will be glad to hear the results; but we should be sorry to make the experiment.

The views contained in *The Human Blight and the Cattle Blight*, are merely assumptions without probability, much less proof.

Dr. Sansom recapitulates the facts as to the history of cholera epidemics, and as to the circumstances which influence its diffusion, which are generally accepted; he urges the use of disinfectants, of which he thinks carbolic acid the best. He then gives us his theory of the nature of cholera; he believes "that cholera is the result of a poison which manifests its effects in two different ways, according as it is in immediate relation with the alimentary canal, or absorbed into the circulating blood. As a primary irritant it causes the immediate phenomena of vomiting and purging; as an absorbed poison it compromises the integrity of the great sympathetic nerve, ultimately becomes an irritant thereof, and causes the combined phenomena of true cholera—diarrhea and collapse." He maintains that the condition of the arteries shows that the poison acts on the sympathetic not as a paralyzer but as an irritant, keeping up a tonic spasm of their coats. Carrying his faith in antiseptics into the treatment, he proposes for choleraic diarrhea either sulphite of soda in half drachm doses, every two or three hours, or carbolic acid in two-drop doses, with three drops of chloroform. When collapse comes on, he adds to these means counter-irritation of the epigastrium by carbolic acid; hot enemata, venesection, injections into the veins, and the inhalation of chloroform, with the intention of relieving spasms. To these he adds the usual advice as to nursing. He gives two cases treated by sulphite of soda; in both the result was favourable; but beyond these we gather that, as yet, he is not in a position to recommend his treatment on other than theoretical grounds.

Note-Book of Materia Medica, Pharmacology, and Therapeutics.

By R. E. SCORESBY-JACKSON, M.D., F.R.S.E., Fellow of the Royal College of Physicians; Physician to the Royal Infirmary, and Lecturer on Clinical Medicine; Lecturer on Materia Medica and Therapeutics at Surgeons' Hall; Examiner in Materia Medica and Medical Jurisprudence to the University of St. Andrews, &c. Edinburgh: Maclachlan and Stewart. London: Robert Hardwicke. Dublin: Fannin & Co. 1866. 8vo., pp. 632.

In compiling the work before us, we believe that Dr. Scoresby Jackson has conferred a great benefit, more particularly indeed on the student portion of the medical profession, but to a large extent on practitioners also. That the book is one especially adapted to

the wants of the former class may be anticipated from the fact that it owes its origin to a desire on the part of the author "to relieve the students attending his lectures as much as possible from the irksomeness of note-taking." Within, too, its necessarily moderate limits is contained all the information on the important subject of which it treats which the practising physician can require. And it is in this attempt to meet the wants of all—as well graduates as students—that we feel bound to congratulate Dr. Scoresby-Jackson on his well-merited success.

Former writers on the same subject, in their laudable anxiety to treat as fully as possible of the more important province of *Materia Medica*, namely, the action and uses of medicines, were too apt, we think, to omit altogether any consideration of what we may call the A B C of the subject. By this we mean the definitions and axioms on which it is grounded; and without a correct knowledge of these it is simply impossible for the pupil to proceed in the study of *Materia Medica*. Now this want has been fully met in the first division of Dr. Scoresby-Jackson's most useful manual.

The work is divided into three parts. The first of these is essentially introductory, and contains: 1st. All necessary definitions; 2nd. A tabular arrangement of "Officinal Formulæ" under the various headings "Aquæ," "Decocta," "Extracta," "Tincturæ," &c.; and 3rd. A treatise on "Magistral Formulæ or Prescriptions," wherein the author briefly but accurately considers: "1. The meaning of the terms *properties, forces, actions, and effects, of medicines*; 2. The *modus operandi* and *classification* of medicines; 3. The *locality* of the action of medicines; 4. The several methods of *applying* medicines to the patient; 5. The *circumstances* which *modify* the actions of medicines; and 6. *the construction of the prescription.*"

In Parts II. and III. we find *inorganic* and *organic* *Materia Medica*, respectively, dealt with in a manner which leaves nothing to be desired. Under each article we have arranged its mode of preparation, with the *rationale* thereof; its characters, tests, uses, doses, and (in the case of poisons) antidotes. The "Organic *Materia Medica*" the author tabulates in three divisions according as the substance is derived from the vegetable kingdom, the animal kingdom, or is the product of fermentation, destructive distillation, &c. Products of the vegetable kingdom are ranged under the natural orders, whence they are derived; and the botanical characteristics of the trees or plants yielding them are given, as are also those of the natural orders themselves.

At the end of the book we find the three appendices which occur in the British Pharmacopœia, and under the title of "index," a complete posological table of all substances used as medicines, the officinal being distinguished by an asterisk.

It will thus be seen that every department of the subject is treated of, and—if not entirely exhausted—is not so because the author, as he states in his preface, sends forth his work as a "Note-Book," and its aim he intends to be "suggestive rather than dogmatic."

The volume is of convenient size, the letter-press good and extremely accurate, and we feel we are but doing our duty in sincerely recommending the work to every member of the profession, be he student or physician.

Memoir of Sir Patrick Dun (Knt.), M.D., M.P.; Physician-General to the Army; and Sometime President of the College of Physicians; Including his Will, his Deed for Constituting a Professor of Physic; and other Important Records Concerning the Profession of Physic in Ireland, never before Published. By T. W. BELCHER, M.D., Dub.; B.M. and M.A., Oxon. and Dublin; Fellow, Censor, Examiner, and Hon. Librarian King and Queen's College of Physicians in Ireland, &c. Second Edition—Revised and Enlarged. Dublin: Hodges, Smith, and Co.

THIS is a second edition of the Memoir of Sir Patrick Dun, that appeared in our last Number, from the pen of Dr. Belcher. It is now published, on the motion of Dr. Stokes and Sir Dominic J. Corrigan, at the request of the College of Physicians, and it has been revised, corrected, and added to by the author, so as to constitute a new edition. The very numerous and interesting facts collected by Dr. Belcher throw a great and unexpected light on the history of the College of Physicians in Ireland, and of physic itself. We congratulate the college on having their early history so fully illustrated, and on having within their ranks one so learned and so industrious as the able and distinguished author of the Memoirs of Dr. Stearne and of Sir Patrick Dun.

1. *A Treatise on the Construction, Proper Use, and Capabilities of Smith, Beck, and Beck's Achromatic Microscope.* By RICHARD BECK. London: J. V. Voorst. Pp. 144.
2. *Smith, Beck, and Beck's Illustrated Catalogue.*
3. *An Elementary Text-Book of the Microscope; Including a Description of the Methods of Preparing and Mounting Objects, &c.* By J. W. Griffith, M.D., F.L.S., &c.; Member of the Royal College of Physicians; Conjoint Author of the Microscopic Dictionary, &c. With twelve coloured plates, containing 451 figures. London: John Van Voorst, Paternoster row, 1864.

THE first of these works is a very perfect and beautifully-illustrated description of the various forms of the achromatic microscope as brought to its present perfection. The term microscope must be taken in its widest sense as including not only the instrument itself, but all the scientific and mechanical apparatus needed for its widest application. We have in these pages clear and succinct descriptions of every kind of microscope from the most elementary to the most complex. The author also displays a just appreciation of the value of many of the more recent additions, such, for example, as Wenham's binocular, which has been at one time lauded as the greatest possible improvement, and again decried as disappointing and incorrect. We shall extract the part referring to this subject as a specimen of the literary merit of the work:—

“Wenham's binocular microscope is now so well known, that it may appear hardly necessary to occupy a portion of these pages with any explanation or defence of its principle; but binocular vision itself, as well as this particular application of it, still meets with such strong opposition from many microscopists, that we venture to give with some minuteness a few facts connected with the subject, and, more especially, because we consider Wenham's binocular body to be the most valuable addition to the compound microscope since the perfecting of the object-glasses by Mr. Lister.

“That binocular vision is advantageous, if not necessary, is quite sufficiently proved by the fact of our having two eyes, and it is also known that the stereoscopic effect conveyed to the mind increases as the object approaches the eyes; consequently, when the simple design of the microscope is to enable the eyes to see an object, or the image of an

object, at a shorter distance than the natural limit, under such circumstances stereoscopic vision must be of the greatest importance.

“In Mr. Wenham’s contrivance, the binocular body is merely an addition; when adapted to an instrument, it does not interfere in any way with the use of the single body only, in its perfect condition. This is a most important feature; for it also affords the opportunity of an immediate comparison between the appearances seen by single or double vision; and thus not only can the accuracy of the binocular body be easily tested, but the observer may obtain in an instant a double analysis of the structure or condition of an object.

“We believe most of the opponents of the binocular principle to be those observers who have accustomed themselves to the examination of objects as transparent; and we venture to draw especial attention to this particular condition, under which so many judge of the value of the Binocular Microscope. This method of examination, peculiar to the microscope, and one to which the naked eye is quite unaccustomed, conveys to the mind a most imperfect idea of the form or vertical distance of the various parts of an object, whilst in very many instances the appearances bear no resemblance even in shape to the true structure.”

In Smith and Beck’s Illustrated Catalogue we have the necessary complement to the preceding work. It gives the prices of every variety of instrument from the humble pocket lens and simple dissecting microscope up to the first-class binocular. A perfect marvel of cheapness and efficiency is the little instrument entitled the universal microscope, which, for a sum of five pounds, comprises all that an ordinary student need require. The educational and the popular microscopes are higher varieties, and are really excellent instruments, to which at any time the most expensive object glasses might be applied. Their prices range from £10 to £15, or £20. For the higher classed instruments and the innumerable paraphernalia which necessity or fancy has originated, we must refer to the catalogue, merely stating that any one so disposed can get rid of a couple of hundred pounds with the greatest ease in the process of acquiring them.

The third book on our list is an admirable companion for those who wish to make a pleasant and profitable use of the microscope. The descriptions, whether of the structure of the instrument, the mounting of objects, or their delineation and nature, is simple, and so far as we have been able to test it, accurate. Into the twelve plates are crowded great numbers of objects, drawn with much accuracy and appropriately coloured. Underneath each is given in figures the

amount by which it is magnified in linear diameter. Any student who works through this book will have acquired a very extensive groundwork of microscopic lore on which to build any superstructure of a special nature that he may choose to apply himself to. The objects described and figured through these pages will, by their variety, lead him on to a thorough management of the instrument and knowledge of its powers.

Army Hygiene. By CHARLES ALEXANDER GORDON, M.D.; C.B.; Deputy-Inspector-General of Hospitals, Army Medical Department; Member of the Sanitary Commission for Bengal; Author of *China, from a Medical Point of View*; &c., &c. 1866. London: John Churchill and Son, New Burlington-street. Calcutta: R. C. Lepage and Co. 1 Vol. Pp. 532.

DR. GORDON, in his preface, states that "among the numerous works that have of late years treated of hygiene, there are none devoted solely and entirely to the subject as it refers to troops in motion and on service." To supply this deficiency is the object of the work under notice. Without its being considered necessary to repeat what has been said by numerous previous writers on the subject of the soldier's barrack life at home, Dr. Gordon proceeds to say:—

"It is, however, but a very partial view to take of the circumstances of a soldier's life, to consider them as requiring no further measures to maintain him effective, and to provide for his necessities, while suffering from the casualties incidental to his profession, than what are applicable to and sufficient for the conditions of civil life. This will, it is believed, be clearly shown in the present volume; and it will, I trust, be rendered equally apparent, that while to meet these requirements a knowledge of the ordinary subjects that constitute modern hygiene is no less essential to the army medical officer than to the private practitioner—the former in order successfully to provide for, and meet the requirements of armies during times of war, must be endowed with a knowledge of dealing with masses in motion, and power of rapidly devising measures to meet emergencies—as well as a capacity for organization that in the latter are never required."

Dr. Gordon then proceeds to demonstrate how far the duties of the military medical officer in a garrison town in the United

Kingdom are identical with those of the civil practitioner, and continues:—

“It is far otherwise, however, on foreign or on active service; and inasmuch as these constitute what may be looked upon as the normal circumstances of the army, we have a right to assume that all arrangements connected therewith should be established with special reference to them. It is accordingly in such conditions that the degrees of scientific knowledge, readiness of resource, and capabilities of army medical officers manifest themselves, either to the unspeakable benefit of individuals as well as the masses generally; or, it may unfortunately be, to the loss of life of the one, and utter disorganization of the other. Thus, then, while upon the one hand it is the direct interest of the state to obtain for its armies the greatest possible amount of talent, it is no less the imperative duty of young professional men who enter this branch of the public service, sedulously to qualify themselves for the faithful and efficient discharge of the important duties that fall to their share.”

Dr. Gordon next alludes to instances “in which the efficiency of an army before the enemy has been maintained solely by the exertions of its medical officers,” and states that “throughout the published accounts of the peninsular war the attentive student will not fail to note some in which the decision of a commander to accept or refuse battle was based upon circumstances connected solely with the medical department:—

“It would add needlessly to the length of these remarks were extracts to be given from such accounts. There is one illustration, however, which in duty to the army medical department, it is but right that I should chronicle in a work devoted, as this is intended to be, to aid its officers in their important duties. It is this:—

“Sir James M'Grigor gives a remarkable example of the extent to which an efficient medical staff may conduce to the success of military operations. He states that, ‘it was said with much truth by an eminent individual, that he thought the extraordinary exertions of the medical officers of the army might be said to have decided the day at Vittoria; for their exertions had undoubtedly added a full division to the strength of Lord Wellington's army; and without those 4,000 or 5,000 men it is more than doubtful if his lordship, with all his unrivalled talents, could have carried the day.’—*Autobiography*, p. 331.”

Dr. Gordon next notices the important services rendered to medical science by military medical officers in times of peace, from

1563, when Thomas Gale, who appears to have been the first systematic British writer on matters connected with military medicine and surgery, treated on gun-shot wounds, down to the present time, showing "that they have not been behind-hand in suggesting those improvements in diagnosis and treatment of disease which have rendered the present state of medical science so greatly in advance of what it was three centuries ago."

A few of the instances of attention to sanitary matters by the older military medical writers, adduced by Dr. Gordon, may be interesting to our readers:—

"Dr. Brocklesby, an army surgeon of eminence, who wrote in 1764, condemned some of the barracks then existing at Chatham as being low and ill ventilated, and describes the hospitals into which sick soldiers were taken as calculated rather to generate than cure diseases. 'These and the barracks,' he says, 'sweep off the men like a perpetual pestilence.'"

When Miss Nightingale made use of somewhat similar language, people in general, and the higher military authorities in particular, were inclined to exclaim, "A Daniel come to judgment," and the administrators of our military system took advantage of the cry, to cover their own delinquencies in having so persistently neglected and set aside numerous warnings from army surgeons of later days, similar to that above quoted, and made her the ostensible originator of all the lately-introduced modifications and improvements in our army hospital system.

Dr. Gordon gives other similar instances which are of considerable general interest during the present revival of attention to sanitary matters. Dr. Nash urges medical officers to "attend to everything which reason and convenience tell them are necessary for the preservation" of the health and lives of soldiers.

"Sir James M'Grigor.—The great administrative talents of the late Sir James M'Grigor are acknowledged, and the great care bestowed by him upon the efficiency of the army is still spoken of with admiration. One of the measures adopted by him is precisely that which some recent writers would seem to have but just discovered. For instance, wherever, during the peninsular war, a general hospital was established under his orders separate wards were set apart for particular diseases, and separate buildings for surgical cases. Not only had each hospital its wards for convalescents, but the convalescents were removed weekly to a convalescent hospital, which, for the most part was some distance in the country."

“Dr. Millingen.—Even the formation of an army medical school is no new idea, for, in 1818, Dr. Millingen published a proposal that one should be established. As regards professors, he suggested the expediency of appointing professors of the following branches of the medical sciences, to be selected from the most able medical officers; namely, theory and practice of medicine; theory and practice of surgery; military hygiene; morbid anatomy, and botany.”

Truly, there is nothing new under the sun!

“Dr. Jackson.—‘The preservation of the health of soldiers in the field and in quarters’—so wrote Robert Jackson, in 1824—‘is attained to a certain extent by a rigid observance of those forms of discipline and economy which are under the direction and surveillance of military officers.’”

“In India.—An administrative medical officer of the Army Medical Department was first sent to that country so as to arrive early in 1826.”

Dr. Gordon gives a short *résumé* of the labours of the first Inspector-General of Hospitals of the British Army in India, in inquiries into the great mortality of newly arrived troops, its rate, and probable causes, from defects of clothing, food, lodging, and discipline. Next, a report was furnished to the Commander-in-chief in India, with a view to establishing a uniform system of clothing, dieting, and exercise for troops in that country. Next, the spirit ration was considered, as also “the proposed plan of substituting for it an equivalent in money, and for establishing canteens in which men might be supplied, under strict regulations, with certain quantities of wine or malt liquors.”

“In 1828 a circular was, by the P M O, addressed to all army medical officers in the country, containing many most valuable directions in regard to the manner of proceeding on the occasion of cholera appearing as an epidemic.

“The advantage of early treatment of the attack was strongly inculcated; a medical officer was directed to reside in the hospital; daily inspection of the men was directed; and the concluding sentence of the circular so strongly prefigures the most recent recommendations that it deserves to be noticed here; it is dated 1st May, 1828. ‘The occasional local origin of the epidemic must be particularly watched and attended to. In some instances lately it has been traced to originate and be present in one wing, or in two or three companies, or at one end of a barrack, as more exposed to the influence of miasma, or the air from foul drains, ditches,

and stagnant water ; upon the removal of the men from which places there was an immediate cessation of the disease.'

"It will thus be seen that the prevention of disease has, throughout all ages, been carefully attended to, and the annals of the British army prove how active and important a part in this matter army medical officers have ever taken, from the time that a standing army was first formed. That this was very often under great discouragement is the experience of the whole generation of army medical officers, whether on full pay or in retirement ; and there are many who can still relate acts of harshness to which they have been subjected for persistently and fearlessly representing matters which, although considered conducive to military discipline, were grievously injurious to health and life of the troops."

Thus does Dr. Gordon in his first chapter furnish to the military medical officer the means of refuting, by the unimpeachable testimony of numerous references to, and extracts from, the writings of British medical officers, the many and serious charges of ignorance and negligence made against their department at the time that the disasters of the first Crimean Winter excited so much popular indignation, and for the quelling of which the whole medical department of the British army was made the subject of undeserved obloquy and reproach, in order to cover the shortcomings of a system of military administration which had, by restrictions and rebuffs, so completely shackled and cramped the members of the administrative and executive ranks of the medical department, and had reduced that branch of the service to such a short-handed state, that, when emergency arose, disorder and confusion were the inevitable results, and it was even found necessary to supplement the military medical ranks with civil practitioners, as physicians, surgeons, and pathologists, and with junior medical men as acting assistant surgeons.

As a proof that the army medical department is still the subject of remarks of a disparaging nature by writers on military matters, it will be sufficient to insert the following extract from an article on "The Aldershot Question," in the *United Service Gazette* of 15th September, 1866:—"When, at the commencement of the Crimean war, Miss Nightingale fell like a shell amongst the military doctors, and declared, among other startling things, that scarlet fever, measles, and small-pox were produced and created by overcrowding and bad ventilation, a universal shout of derision from the professionals was all the benevolent lady got for her pains. It was held that those three plagues of infancy came as naturally as the

teeth, and that any attempt to prevent their occurrence by sanitary measures was sheer nonsense."

How unjust such aspersions are Dr. Gordon's book fully shows, and since its publication a repetition of such charges can only occur in consequence of a degree of ignorance no longer excusable on the part of any writer on military sanitary questions.

Dr. Gordon devotes Chapter VI. to the subject of ships; and we can confidently state that in this chapter will be found a mass of information on every subject connected with troops on board ship, such as could only have been furnished by a person in the author's position, and habitually and practically conversant with the subject; to the junior medical officer, proceeding on his first voyage, this chapter will be invaluable, and we would strongly recommend to the military medical authorities that this book, were it for this chapter alone, be classed among the text books essential to the outfit of every assistant surgeon entering the service.* In this chapter the author gives a code of rules, which he drew up for the instruction of medical officers arriving at Calcutta with troops, and mentions that they have since been embodied in a circular, dated Army Medical Department, 15th January, 1865.

Chapter IX is devoted to the subject of army hospitals. Dr. Gordon describes the three kinds of hospitals necessary on service, viz.: first, regimental; second, field general hospital; and, third, purely general hospital. They are all necessary; and Dr. Gordon deprecates the tendency which at present exists to the centralization and absorption of the regimental and field hospitals in the general hospital; with respect to the present system of general hospitals, in which the responsibility is divided between the medical officers, the military commandants, and the purveyors, Dr. Gordon observes:—

"The medical officer in charge is the responsible agent for all requirements of the sick; yet under the present system he himself is deprived of the power of ordering the very requirements which he may deem indispensable for them; being checked, on the one hand, by the military commandant, on the other, by a purveyor; both of whom, be it remembered, are unqualified, by absence of medical training, for the performance of duties they have actually to superintend; yet having no personal responsibility for the failure of them."

* A drawing is given of sections of a ship illustrating the mode of calculating space for troops, and rendering this very important point perfectly clear.

In fact, as it appears to us, the medical officers of the army stand in much the same position with regard to commandants and purveyors as do union medical officers in this country to boards of guardians, and, as is well known, in the latter case opinions occasionally differ, with what result to the sick we need hardly mention.

On one subject—and we may say only one—connected with military hospitals, is our author silent, and that is on *female nurses*. Are we right in attributing this to a polite reticence on the part of one who, as we have above shown, does not hesitate to speak out boldly enough on other subjects? How far may this omission be accounted for by the extremely anomalous system by which the department of female nurses in military hospitals is now administered? a system presided over by a lady “Superintendent-General of Nurses,” who, though theoretically subordinate to the medical officers, is practically in a superior position, from the fact of having the privilege, as “head of a department,” of communicating direct with the Secretary of State for War, a privilege which, we need hardly say, is possessed neither by principal medical officers, commandants, nor purveyors of general military hospitals.

Chapters X. and XI. contain full information on the means of transport for sick and wounded, and on the establishments and equipment for sick and wounded; and Chapter XII. gives some illustrations of the rates of sick and wounded on service. Chapter XIII. treats on medical arrangements generally for a campaign; this chapter we consider will be found by the senior medical officer fully as valuable as we have pronounced Chapter IX., on ships, to be to the junior. In it will be found, as indeed throughout the whole work, such attention to points of most minute detail as must be invaluablely suggestive to the military surgeon, which clearly indicates how profoundly the author has considered his subject, and how high is the degree of administrative talent which he possesses, and which we doubt not, when occasion arises, will add to the honours and position which he has already so deservedly earned in the Army Medical Department; the advice to the medical officer “who would succeed” is so applicable to all classes that we extract it:—

“Also to bear in mind that his own superiors and the public look more to the fact of certain results having been attained, than to the means employed to a specific end. These means must be straightforward and open; yet the medical, or indeed any other officer who would succeed,

must be prepared at all risks to do so; and to sink all considerations of a minor nature in the one great object of contributing, as far as in him lies, to the success of the service upon which he is about to enter."

We must now draw to a close our already lengthened notice of this work; we congratulate the medical department of the army on this addition to its literature; and we believe that if such knowledge of the true dignity and position of the army surgeon, as may be deduced from it, were more generally diffused among military medical officers and their combatant brother officers, many of the petty bickerings and irritating discussions which appear so frequently in military and medical publications about precedence, relative rank, &c., would soon cease to exist; and that Dr. Gordon has rendered true service to the department to which he belongs, by the dignified and dispassionate manner in which he alludes to many vexed points in the mutual relations of the commanding military officers and their medical coadjutors; he does so, incidentally, as it were, on questions obviously and naturally rising out of his subject, and markedly avoids founding thereon any querulous appeal or remonstrance on the state of these relations as they at present exist; he prefers to indicate what they ought to be by illustration, as for instance, the Duke of Wellington frequently communicating to Sir James M'Grigor the intended movements of the army—and as a particular instance mentions the beneficial results of this system in the thoroughly effective departmental arrangements which Sir James M'Grigor was thereby enabled to make in anticipation of the attack on Badajos; he also specifies a personal instance of the advantages of a similar system while he was serving as senior medical officer of the force under Major-General Sir Edward Lugard, K.C.B., during the operations against the Indian rebels in 1858.

From a perusal of this work may be perceived the great importance of the subject on which it treats, and how worthy of the most strenuous effort are the objects of ambition which are therein held forth for the attainment of the aspiring army surgeon.

The circumstances under which the book was written explain its having so much the character of a collection of aphorisms—namely, the attention of the author as member of the Sanitary Commission for Bengal, having been specially directed to the suggestion of improvements in the condition of our troops in that country, it was necessary to refer to many works connected with

this and allied subjects, he was thus led to take notes, which together with the results of his own experience, assumed on arrangement their present shape.

The book has been printed in Calcutta, and published in London; this is to be regretted, as the paper and type are both inferior to those with which Mr. Churchill generally favours his customers; and from superfluous margin not having been trimmed off, the external appearance suggests the idea of the book being larger than it is found to be on internal inspection. There are also a few typographical errors of spelling and punctuation, which, however, are of minor importance, and in no way detract from the true merits of the work, which we strongly recommend to the study of our military readers.

Fissure of the Soft and Hard Palate. By J. MASON WARREN, M.D.; Surgeon at Massachusetts's General Hospital, &c. (Extracted from the *Transactions of the American Medical Association.*) Two Plates. Philadelphia: Collins. Pamphlet.

WE hail with pleasure this further contribution from the pen of one who has been for a quarter of a century the successful operator in the New World on cases of fissured palate. Mr. Mason Warren was the first man to recognize the fact that fissures in the hard palate were the result rather of misplaced than deficient bone, and to apply to their remedy the obvious method of dissecting off the muco-periosteum from either side, and uniting its edges at a lower level. In short, he put what may be termed a false ceiling to the mouth. This ingenious method approved itself to Avery and Pollock of London, to Field of Brighton, and to Maurice Collis of Dublin, all of whose names are gratefully mentioned in the present paper by Warren, as supporting his claims to priority against Langenbeck. This last surgeon had, in 1862 (nineteen years after Warren's first publication), put forward an identical process as a new invention of his own, apparently in happy ignorance of what Warren had done. The readers of this Journal are aware that Messrs. Lestrangle and Collis had also planned a similar operation somewhere about 1845, without a knowledge of Warren's work; but they have always yielded the palm to Warren, to whom the priority of the idea is justly due. Mr. Warren, in this pamphlet,

shows himself fully alive to all that has been written on the subject; and the various improvements in the performance of the operation which have emanated from Dublin surgeons are well known and highly valued by him; among these we find the suggestions of Sir Philip Crampton, Mr. Collis, and Mr. Smyly prominent. Mr. Warren's acquaintance with Irish surgery might be a lesson to many at home who remain habitually ignorant of the doings of their brethren.

Mr. Warren relies principally on the curved scissors for division of the palatine muscles and mucous membrane. By seizing the half uvula, and drawing it across the cleft, the palato-pharyngeus is put on the stretch; it is then divided, and the levator palati then appears as a horizontal bar above and behind the palate; this also is divided by one or more strokes of the scissors, until all resisting bands of muscle or mucous membrane are severed and the palate hangs as a loose curtain. By this means Mr. Warren thinks perfect division of all resisting bands is obtained, and nothing is divided unnecessarily; whereas by Fergusson's method (difficult and uncertain as it must always be) an unvarying amount of muscular tissue is divided, without reference to the necessities of each case. Such is Mr. Warren's view of the incisions required. We have only to add that he always separates the muco-periosteum first, before dealing with the soft palate or its muscles, and that he calls in the aid of the mechanical dentist to cover with vulcanite or gold the anterior portions of the cleft in the hard palate. One of his reasons for so doing does not hold good, namely, that these operations must be done without chloroform. We have twice seen Mr. Collis operate on the palate while the patient was fully chloroformed, and on neither occasion was there any trouble from the blood, it was removed quite as easily as when the patient was conscious; and the operation was done at much greater ease to the surgeon, and more rapidly than under other circumstances. Still there is no doubt it is a severe operation, and takes a great deal out of a patient; and when a plate will do as well to close the hard palate, we should prefer it.

The only remaining point in Mr. Warren's paper which calls for observation is that he prepares his ligatures of silk by soaking them for a few days in compound tincture of benzoin, by which they acquire an adhesive property, and are less apt to slip when the ligatures are tied. This improvement he owes to a former pupil, Dr. Calvin Page.

The Malformations, Diseases, and Injuries of the Fingers and Toes, and their Surgical Treatment. By THOMAS ANNANDALE, F.R.C.S., Edin. Edinburgh: Edmonston & Douglas, 8vo, pp. 292.

THE book we now bring before our readers has been the successful essay for the Jacksonian prize of 1864, and afterwards published, with considerable additions, at Edinburgh in its present form.

We are happy in congratulating Mr. Annandale on his success, and consider that his labour and zeal for the development of the subject he has adopted have not been too highly rewarded.

The first chapter is devoted to the consideration of the congenital malformations of the fingers and toes, under the following six heads:—

1. Congenital Hypertrophy.
2. Congenital Deficiencies.
3. Supernumerary Fingers and Toes.
4. Congenital Union.
5. Congenital Contraction.
6. Congenital Tumours.

The author treats in detail of the various subjects, illustrating them by lithographs and drawings, taken principally from specimens in his own possession, the Museum of Newcastle-on-Tyne Infirmary, and Edinburgh University.

The division of supernumerary fingers and toes given in this chapter is the most practical we know of, as it points anatomically to the exact condition present, and guides us in the operative measures for the remedy of such condition. It is as follows:—

“1st. As a deficient organ attached loosely or by a narrow pedicle to the hand, foot, or another digit.

“2nd. As a more or less developed organ, free at its extremity, and articulating with the head or sides of a metacarpal, metatarsal, or phalangeal bone, which is common to it and another digit.

“3rd. As a fully developed organ, having its own metacarpal or metatarsal and phalangeal bones distinct.

“4th. As a more or less developed organ intimately united along its whole length to another digit, and having either an additional metacarpal or metatarsal bone of its own, or articulating with the head of one which is common to it and the other digit.”

The probability of these deformities being hereditary is glanced

at in page 2, and the Cases 12 and 13 furnish us with a strong presumption in its favour.

The treatment recommended for the relief of those conditions is simple, and based on the soundest surgical principles. At page 58 a diagram is given illustrating an original and ingenious method of operating on webbed fingers and toes of the second, variety on the principle first suggested by the late Mr. Liston:—

“The surgical treatment of the second variety of congenital union has not hitherto been satisfactory, the intimate connexion of the soft parts in such cases making it very difficult to prevent re-union taking place after operations have been performed to separate the bond of union. The treatment, however, that I am now going to describe has proved very successful, and I venture to hope that it will be of service in cases of this deformity. The principle of this operation is not new, but the method of its performance is, as far as I am aware, original. This plan of operating is mentioned by the late Mr. Liston,^a but it does not appear ever to have been practised until lately, when Professor Lister of Glasgow treated a case of united digits in this way. The operation consists in making a perforation through the proximal end of the web or band of union, introducing some foreign body into the wound, and retaining it there until the edges of the opening are cicatrized, or nearly so; the remainder of the union is then to be divided, and the wounds carefully dressed. By a proper employment of this treatment there is little chance of the separated digits re-uniting; and Professor Lister informs me that the result of a case on which he operated after this manner has been most successful. The following method of performing this operation will, I think, be found most convenient.

“Introduce a trochar and canula (a No. 4 canula answers very well) through the web or union which connects the two digits, at a point where the cleft between these two organs would naturally be. Having thus made an opening completely through the web, the trochar is to be withdrawn, and a piece of Indian-rubber cord, about the thickness of a No. 8 catheter, passed through the canula; the canula may now be removed, leaving the cord in the wound; one end of the cord is to be carried along the dorsum of the hand, and secured to a band passed round the wrist, the other end is to be carried along the palm of the hand, and secured at the wrist in the same manner, as is illustrated in the diagram. The advantage of this plan is that the cord being soft and elastic, allows the hand to move freely without disarranging the apparatus, while at the same time it also acts as an efficient foreign body. In cases where the union is not quite so firm, I have divided the

^a Operative Surgery, by Liston, p. 474.

whole union at once, separated the two layers of the skin at the side of the connected digits, brought the edges of these wounds together with sutures, and then introduced the elastic cord between the divided fingers, adjusting it in the same way at the wrist. This treatment prevents the wounds at the side of the fingers re-uniting, and keeps up a gentle pressure on the lower part of the cleft. Even in this method of operating great care is required in conducting the after treatment."

Chapter II. treats of inflammatory diseases of the digits, the following order being observed:—

1. Acute inflammation affecting the skin and areolar tissue.
2. Acute inflammation affecting the tendons and their sheaths.
3. Acute inflammation affecting the periosteum and bone.

The different forms of paronychia are described according to the tissue attacked. But nothing has been added to the pathology of this disease, except that our author found by dissection that in many of the worst varieties of whitlow attacking the tendons and their sheaths, the joints remain unaffected.

In the treatment of this affection we find nothing newer than Dupuytren has taught many years ago, nor is there any rule laid down to guide us in the extent and depth of our incision in the second and third varieties of this inflammation. The rule adopted in the Dublin School of Surgery is to cut down to the bone in the mesian line, avoiding the joints, as we never can tell when the inflammation of one structure has travelled to and implicated its neighbour.

The paragraph on horny hypertrophy of the nails is very brief, and gives no treatment. We consider the operation proposed and practised by Mr. George Lewis Cooper, and described by him in the *Lancet* for 15th April, 1866, the most suitable to the affection. The mode of proceeding is as follows:—Divide the matrix in the centre, and slightly loosen the horny structure from its attachment, then by a sudden evulsion it comes away.

The pathology of in-growing toe-nails is well described. But we cannot agree with Mr. Annandale that the operative measure proposed by Dupuytren for its relief is the best, viz., to divide the nail longitudinally down to the root, and then, with a pair of forceps, tear out the portion of it which lies against the sore.

In the first place, the operation is most painful if the patient will not submit to chloroform; secondly, the after-dressing of a naked matrix is excessively painful.

We prefer the method practised by the late Mr. Smyly, of this

city. The patient being placed under the influence of chloroform, he proceeded then to remove the entire offending mass with one clean cut of a strong scissors. This perfectly relieves the affection, and the after-dressing is painless, as there is no matrix exposed.

Chapter IV. treats of tumours of the hard and soft textures of the digits. We would especially call attention to the section on enchondroma, the pathology and treatment of which is fully entered into.

Chapter V.—In this chapter injuries of the digits are considered under the following heads:—

1. Injuries of the Soft Parts.
3. Injuries of the Bursæ and Joints.
3. Injuries of the Periosteum and Bone.

At page 203 a most interesting case is given of a wound of the digital nerve of the left ring finger, followed by paralysis, burning sensation, and other abnormal symptoms. This case is the more interesting because of the importance attached to injuries of the nerves during the late American campaign. It is in the neuralgic pain attending those injuries that the American surgeons have been so successful with the hypodermic injection of the salts of morphia.

“CASE 35.—Wound of a Digital Nerve, followed by Paralysis, Burning Sensations, and other Abnormal Symptoms.

“Mrs. —, aged sixty-four, applied to Mr. Syme, June 28, 1865, for a very painful complaint of one finger. Nine months ago, the patient accidentally inflicted, with a knife, a wound over the palmar and lateral aspect of the first phalanx of the ring finger of the left hand. The wound healed quickly, and without any inconvenience. One month after the accident (the wound then being perfectly healed) she first began to suffer from pain in the cicatrix, and round about it. This part became very sensitive, so that the slightest touch caused great agony. Some swelling of the fingers and hand then took place, and the pain, and a burning sensation, passed up into the hand, forearm, and elbow, occasionally also shooting up into the arm. At this time her medical attendant made an incision near the cicatrix, but it was followed by only a very slight temporary relief. Blisters were also employed, but without any benefit resulting. The burning sensation was now not only confined to the affected arm and hand, but was often very severe in the right hand and forearm, and also sometimes in the soles of both feet. She then placed herself under the care of an eminent physician, who treated her most carefully with a variety of local applications, among which were chloroform, belladonna,

aconite, etc. Not obtaining relief from these means, she came to Edinburgh for advice.

“On examination of the left arm, the elbow, wrist, and digital joints were stiff and painful. The fingers were tapered, and their skin had the characteristic glazed or glossy appearance (which was first observed by Mr. Paget in cases of injury of the nerves) well marked. The colour of the skin was pale, with a slightly yellowish tinge. The nails were dry and brittle. The glossy state of the skin was also present on the dorsal surface of the hand, and for a little distance up the forearm. The left hand and all its digits were very sensitive, but the injured finger was most markedly so, especially at its extremity, and on its palmar aspect. The slightest touch on this finger caused such distressing pain that the patient would scarcely permit the hand to be approached, much less touched. On a careful examination of the affected finger, a cicatrix was distinguished in the situation of the original wound. On pressing over this point great pain was complained of.

“The right hand was also slightly stiff. Its palmar aspect had a shrivelled and wrinkled appearance, but was not tender on pressure. The patient suffers very much from the pain and burning sensation. These symptoms are almost constant, but are more severe at some times than at others. Occasionally they are temporarily suspended, but this does not last long. The pain from which the patient suffers is like that of neuralgia, but it is the peculiar burning sensation of which she complains most. This sensation she compares to the application of a hot iron; and it attacks not only the affected hand but passes up into the forearm and arm of the same side. It is often very severe in the right hand, and is also sometimes present in the soles of her feet, but here it is not so bad as in the hands.

“The patient's general health is apparently good, and there is no history or symptom of any other nervous disease. Her friends think that she is a little more “nervous” than she used to be; but this is scarcely to be wondered at, considering what she has suffered; and, in fact, if she had not had a very good constitution, her general health would not have been so good as it is.

“*June 31st.*—Having carefully considered the case, Mr. Syme thought it right to propose amputation of the affected finger, as the history clearly showed that the symptoms dated from the injury to its nerve, and the extreme sensitiveness of this finger pointed to it as the probable source of the painful symptoms in the other regions. The patient willingly agreed to the operation, and the finger was therefore removed at its metacarpophalangeal joint.

“*July 3rd.*—Patient feels the affected hand much easier, but still has occasional attacks of the burning sensation in the right hand. Finger was dressed for first time. The wound looks well.

“*July 6th.*—Last night the patient had a severe attack of the burning sensation in both hands, but is easier again this morning. Wound is dressed daily, and is healing nicely.

“*July 10th.*—Patient has been gradually improving for the last few days. She is very much better to-day, and is quite free from her old pains. She allowed me to grasp the affected hand freely, move the fingers and wrist-joint, and in fact handle the limb in a way she could not for a moment have borne before the operation. The peculiar glossy state of the skin is not nearly so well marked now. Ligatures have all come away, and the wound is nearly healed.

“*July 15th.*—Since last date patient has had some return of the burning sensation, but it is not so bad as it used to be. She was anxious to return to the country, as the hand was so well, and, accordingly, to-day left town, promising to report progress.

“*August 12th.*—The patient's daughter writes to-day, and states that her mother's general health is much improved. She still suffers occasionally from the burning sensation in the left hand, but it is not so severe as formerly. The wound is quite healed, and the whole hand and arm are more easily moved than before. The burning sensation has, however, increased in intensity in the right hand, which has become somewhat swollen and its joints stiffened.

“*Examination of the Finger after Amputation.*

“Two hours after the finger had been removed, I made a careful dissection of its nerves, and found that the nerve which usually runs along the radial border of the palmar aspect of the digits had been injured at the point corresponding to the cicatrix of the original wound. At this point the nerve had been apparently partially divided, and had become re-united. There was an enlargement of the nerve at the situation of the injury. This enlargement of the nerve extended for about the eighth of an inch above and below where it had been wounded. In appearance it was more vascular than the other nervous tissue, and looked as if suffering from some slight inflammatory process. In addition to this condition of the injured nerve, the Paccinian bodies in connexion with the nerves and their branches on the palmar aspect of the finger were particularly well developed. For this reason, I had a careful drawing taken from my dissection of these nerves, and the accompanying woodcut is from this drawing. The line *a* points to the place where the nerve had been injured. The drawing does not represent the whole of the Paccinian bodies which were connected with the nerves of this finger, for, in dissecting them out of the fatty and areolar tissue, a considerable number were lost and destroyed.”

In the thirteenth volume of our first series Mr. Hamilton has

recorded a number of cases very similar to this; and, as his paper is most complete and exhaustive of the subject, we are much surprised Mr. Annandale has not referred to it. In one of the cases mentioned by Mr. Hamilton amputation of the leg was performed with results no more satisfactory than those in the case given by Mr. Annandale. Three of the cases given were treated by mercury—in one relief followed. In another, in which Sir P. Crampton, Sir Astley Cooper, John Bell, Dr. Baillie, and Mr. H. Cline had been consulted as to removing a portion of the nerve, and whose various opinions Mr. Hamilton mentions, the exhibition of mercury for pneumonia, some time after the nerve had been excised by Sir P. Crampton, was followed by complete subsidence of the symptoms.

In the third case the mercury produced no effect, but the termination of it is so remarkable that we shall quote Mr. Hamilton's words in describing it:—

“June 14th, Mr. Quinan told me that some days since she got a fright in the street and fell into a hysterical fit, and had to be carried home; for a couple of days after recovering from it she was affected with a constant sobbing; and when this ceased, hiccup of a most violent kind began, with difficulty of swallowing, and pain and tenderness in the epigastrium, apparently the result of the violent action of the diaphragm. After the fit all pain and swelling left the arm, and have never returned. The sudden violent impression on the nervous system generally, having quite mastered the local affection.”

The conclusions drawn by Mr. Hamilton from his examination of the subject are as follows:—

“1. Pain of the severest character in the injured part, or shooting from it in various directions in the course of the nerves to the extremities, or to the brain. That it is often periodical, and generally attended with the most exquisite tenderness.

“2. That with the pain there is frequently redness and swelling, resembling a good deal the appearance of the skin in inflammation of the fascia, or a deep collection of matter, but differing, in being of a paler colour and more œdematous character, in being subject to sudden increase and diminution, and at times being only periodical.

“3. Contraction of the limb, and spasms of an unusually violent character; the spasms in some instances going into the genuine convulsion of epilepsy.

“4. That the constitutional symptoms are those commonly called

nervous and hysterical, viz.: depression of spirits, prostration of strength, globus, &c., but that hectic fever has also presented itself.

“5. That all the symptoms, uninfluenced by every remedy made use of, have gradually declined after a long interval, having apparently undergone a natural cure. But that in a few instances the termination has been fatal.”

In a case recently under our own observation, in which the symptoms of irritation and burning sensations pervaded the whole body, and were often accompanied by spasms of a tetanic character, we found that these subsided under the use of enforced rest, by means of a splint, and frequent blistering along the course of the brachial plexus of nerves. Altogether, we believe the subject deserving of more attention than it has hitherto received; and that the study of injuries of the fingers and toes is daily becoming more necessary since the introduction of circular saws and such machinery into this country.

In the article on senile gangrene we recognize nothing more modern in the pathology and treatment than what has been taught by Mr. Pott in his *chirurgical works* of 1808.

Mr. Annandale states that this disease depends on a diseased state of the coats of the arteries, while Mr. Henry Lee regards it as a chronic process, and caused by the breaking-up and entrance into the circulation of calcareous, fatty, and other morbid deposits from the inner coats of the vessels, with shreds of fibrin, which may have temporarily attached themselves to the vessel when the lining membrane has given way. Sir B. Brodie believed that the remote cause of this condition depended on ossification of the arteries of the thigh, and leg, and contraction and obliteration; yet he did not think, as others have, that this condition was produced by inflammation, for he mentions that in none of these cases in which the arteries were contracted and impervious, were there any such appearances as would have indicated that these contractions had been the result of inflammation. He accounts for it by supposing it to be a result of a contractile process analogous to that which produces a permanent stricture of the urethra or œsophagus. To inflammation he has ascribed the exciting cause.

Mr. Annandale, in alluding to the treatment of senile gangrene, recommends that soothing poultices, and other applications of this kind, should be made to the gangrenous part. We believe with Sir B. Brodie that poulticing is to be condemned, as it exposes the part to alterations of temperature, being obliged to renew them at

least three times daily. The mode Sir Benjamin prefers is to apply simple calamine cerate to the ulcers, and to the limb, as far as the middle of the thigh, layers of carded wool, sufficient to make the leg seem twice its normal circumference. This dressing is not to be removed more than once a week. The latter part of his treatment he owes to Mr. Vance, formerly surgeon to Greenwich Hospital.

The author, in speaking of the internal remedies, remarks that opium should be given freely in this disease, where there is much pain and restlessness.

As Mr. Pott was the first to introduce this valuable medicine for the relief of the malady, we think it but fair to him to state the precautions he gives when employing it. He says :—" I give one grain of opium every third or fourth hour, never less than three or four grains in the twenty-four hours, taking care to watch its narcotic effects, and keep the belly empty by clysters." Mr. Annandale omits to mention such precaution.

Chapter V. gives a good description of the nature and treatment of that rare disease termed scrivener's palsy, as well as some interesting cases of spasmodic affections of the fingers.

Chapter VI. is devoted to the consideration of an interesting and practical subject, namely, excision of the smaller joints. We must say that it is full of instructive information, and in it are to be found cases of great value to the practical surgeon, fully illustrating the beneficial effects of conservative surgery. We have lately performed the subperiosteal section of Langenbeck on the metacarpo-phalangeal joint, with the happiest results. We subjoin a case which the author illustrates by a diagram at page 262.

"CASE 36.—Excision of the Metacarpo-phalangeal Joints of all the Fingers.

"G. M., aged seven, admitted into the Clinical Surgical Wards of the Royal Infirmary on the 1st of April, 1861, having injured his hand in a saw-mill. On admission, there was a wound across the back of the hand, which had divided the extensor tendons of all the fingers, and had opened into all the metacarpo-phalangeal joints. The first phalanges of the ring and little fingers had also been sawn through, immediately above their proximal articular extremities, so that the anterior portion of the hand was only attached to the posterior portion by little more than the flexor tendons and skin on the palm of the hand. The thumb was uninjured. Thinking it right to give the hand a chance of recovery, I

cut off the articular extremities of the metacarpal and phalangeal bones connected with the metacarpo-phalangeal joints of the four fingers. The hand was then placed on a splint, and light dressing applied. The wound healed well, and on the 8th of May the patient was dismissed quite well.

“On the 2nd of July, 1862, more than a year after the operation, I had an opportunity of examining the hand, and found that its appearance was wonderfully natural. It was slightly shortened, but was quite movable and useful. Complete flexion could not be performed.”

In chapter VII., on amputation of fingers and toes, there is nothing which is to us an improvement.

In conclusion, we have to say that the general arrangement of this book is good; the cases are well and accurately taken; the divisions of the various chapters into sub-sections makes it easy of comprehension, and capable of being retained by the most superficial reader.

The language is simple, yet expressive; evidently showing that the author's object was to picture disease as it really exists, and to make his book capable of being understood, even by a student of one year's standing.

The lithographs are beautiful, and faithfully illustrate almost all the subjects treated off.

We can with confidence recommend this book to our readers as being the most complete on the subject in the English language, and containing nearly everything that requires to be known in its special department.

Symptomatologie und Diagnostik der Hirngeschwülste. Von DR. PAUL LADAME. Würzburg: 1865.

Symptoms and Diagnosis of Tumours of the Brain.

THE author commences by defining the limits of his work. He disclaims any intention of writing a complete monograph on intracranial tumours, but confines himself to merely their symptoms and diagnosis; further, he does not treat of every kind of tumour, but excludes from consideration the following:—1. Tumours which, during their development, break through the cranium, and appear externally; 2. Abscesses; 3. Aneurisms; 4. Cysts, whether simple or connected with solid growths, and whether parasitic or not;

and 5. Syphilitic tumours. Even after these restrictions, a large class of tumours is left, of which he treats from a purely clinical point of view, believing that in most instances it is impossible accurately to diagnose the anatomical nature of the tumour before death.

He endeavours to avoid the mistake committed by many writers of attributing *all* the symptoms observed during the life of the patient to the tumour found *post mortem* in his brain, and tries to discriminate those caused by the new growth from those due to complications affecting either the brain or other parts.

The book is divided into a general and a special part. In the first are given the symptoms which are common to all intracranial tumours, and which distinguish them from other brain diseases.

In the special part, the diagnosis of the situation of the tumour is specially considered: this part is divided into chapters, each of which is devoted to the consideration of the tumours of a different locality. In each chapter is found first, a table containing the cases of tumour occurring in the part under consideration which the author has collected from other writers, arranged so as to give the name of the observer; the age and sex of the patient; the description of the tumour; the symptoms as they showed themselves during life in disturbances of sensibility, motility, special senses and nutrition; and finally, observations on the case.

Then follow the conclusions drawn from the examination of the table; and lastly, the author's own cases are given in detail. These we look on as the most valuable part of the book; the histories are recorded with great care, and each case is followed by an epicrisis or commentary in which are clearly pointed out its most important clinical and physiological features. In all 331 cases are analyzed; of these nine are original.

The early symptoms of a tumour in the brain are very variable, and not at all characteristic. They may set in acutely; but as a rule they commence very gradually, so gradually that in many cases there is great difficulty in determining precisely the exact date of their commencement. The origin of his disease is often attributed by the patient to an injury; but in such cases careful enquiry will frequently discover that, prior to the date of the blow or fall, head symptoms existed, although perhaps in a very slight degree.

The course of the disease is characterized especially by the remittent character of the symptoms, and by their great variety. With regard to the latter peculiarity, the author remarks that we

have to do with neither a single morbid product nor with a single diseased organ; that the brain is in reality a group of organs, the respective functions of which are indeed imperfectly known, but nevertheless distinct; hence, diversity of symptoms should no more cause surprise in brain diseases than it does in diseases of the abdomen, where in one case the liver, in another the stomach, and in a third the kidneys, are the seat of the affection.

Disturbances of sensation are found in almost all cases of intracranial tumour; they generally appear early, and persist through the entire duration of the case; of these headache is the chief. Hasse, indeed, thinks it is never entirely wanting, but the author finds it absent in rather more than one-fourth of his cases. In character it varies greatly—it may be continuous and of great severity; or remittent, presenting exacerbations, in the intervals of which it is slight but never entirely absent; or it may be intermittent, in which case it is liable to be mistaken for ordinary hemicrania. The acute attacks often occur in the night, and are sometimes terminated by vomiting, convulsions, or even death. In the same patient, the headache may be at first intermittent; later it may become remittent, and finally continuous. The kind of pain varies; sharp and cutting, dull and heavy, beating or darting; it may also be felt as a sensation of extreme heat or cold. The position of the pain also varies; it may be felt generally over the entire head, or general at first, it may subsequently become localized in one region, or it may be localized from the first. Its seat does not infallibly point out the position of the intracranial disease. The pain is usually increased by expiratory efforts; but this is not, as Romberg supposed, diagnostic of the headache caused by a tumour. The circumstances which chiefly characterize it are its remissions, severity, and, above all, its obstinate resistance to treatment.

Painful affections of the parts supplied by the fifth nerve are frequently observed. These differ in no appreciable manner from ordinary facial neuralgia; but taken in connexion with other symptoms of intracranial tumour, they are important, as they always indicate a direct implication of the fifth nerve, and hence point out the seat of the disease. The painful parts are very generally, at a later period, deprived of sensibility. Pains in the trunk or limbs are sometimes, but not constantly, felt; they are often aggravated on the approach of bad weather, and may be mistaken for rheumatism. Anesthesia is not unusual, most common in the face, sometimes affecting the limbs, very rarely the entire body. It is

generally preceded by pain, formication, or some other kind of dyesthesia, and frequently accompanied by motor disturbance. Attacks of giddiness or swooning are not infrequent; when accompanied by headache they are of great diagnostic importance.

The disturbances of motility may be divided into two great classes:—1. Those arising from irritation. 2. Those arising from loss of function of some part of the brain. In general they are more common than disturbances of sensation, being met with in five-sixths of the author's cases. They may occur suddenly, and as the first symptoms, but more frequently they come on gradually, and are preceded by headache. Of the symptoms belonging to the first class of motor disturbance the most important are *convulsions*; these may be of every degree of severity, from the twitching of a few muscles to fits of epilepsy; they are sometimes a very early symptom, and sometimes show themselves only a few hours before death; they may recur at variable intervals, and often become more frequent as the disease goes on. They are very generally followed by paralysis, but this does not always stop the convulsive movements which may continue to affect the paralysed parts. Among the less frequent symptoms of the irritative class may be enumerated tonic cramp of the muscles of the neck, or of an extremity; trembling generally of the hands or head; choreic movements; rotation movements; and loss of co-ordinating power, so that, though there may be no actual paralysis, standing or walking is impossible.

In the second class of motor disturbances, or those due to loss of function of parts of the brain, are included the various forms of paralysis; these are as diversified as are the phenomena of the first class. Hemiplegia is the most common form; it may appear suddenly and follow an apoplectiform attack, in which case, in addition to the tumour, there is generally some other brain disease, such as softening or hemorrhage; but most frequently it comes on gradually and develops itself out of a condition of simple weakness. Friedreich has observed that in many cases the face is first affected, then the arm, and lastly the leg. In general, the face is paralysed on the same side as the limbs; when the paralysis is crossed, there is good reason for supposing the pons varolii to be the seat of disease. The facial paralysis is usually of the imperfect kind which co-exists with central disease of the portio dura. Paraplegia is sometimes observed; in these cases the tumour is generally situated in or near to the medulla oblongata. This cerebral paraplegia differs from that produced by spinal disease in affecting the lower limbs, not

simultaneously, but one after the other, and in never being equally complete on both sides. Various forms of local paralysis, as of the face or of a single limb, are met with. The author records a curious instance in which the extensor muscles of the wrists and fingers were alone affected, the case simulating one of lead poisoning. Partial paralysis of the tongue is not infrequent, while complete glossoplegia is very rare. Towards the end of the patient's life involuntary evacuations of urine and feces take place. In one case incontinence of urine was one of the earliest symptoms.

In about one-half of the cases of intracranial tumours collected in the work before us disturbances of the special senses were observed. The sense most frequently affected is that of sight; we find amblyopia or amaurosis coming on gradually or occurring suddenly, generally preceded by headache, and less complete in one eye than in the other. The anatomical changes in the fundus are first, inflammation, and subsequently atrophy of the optic nerve. The pupils are generally dilated, often unequal, rarely contracted. Squint, either paralytic or spasmodic, is not uncommon, and gives rise to diplopia; when the fifth nerve is pressed upon, there is insensibility of the conjunctiva and inflammation of the globe, followed by atrophy.

In the organs of hearing, we notice complete or partial deafness and noises in the ears.

Abnormal conditions of the senses of smell and taste have been seldom noticed; but it is probable that if more carefully sought for they would be more frequently found. When many of the special senses are simultaneously affected we have good reason to suspect a tumour at the base of the brain.

Mental disturbances show themselves in about one-third of the collected case; like the lesions of motility, they are irritative or paralytic: thus we may have hallucinations, monomania, mania, ecstasy; or, on the other hand, hypochondriasis, melancholia, with or without suicidal tendency and idiocy. In some cases the symptoms closely resemble those of general paralysis of the insane.

The powers of speech may be seriously impaired; sometimes the words are pronounced with effort, each syllable being strongly accentuated; sometimes there is stuttering, and sometimes the patient loses the recollection of words, or the power of finding those suitable to express his ideas.

The author utterly disbelieves in the localization of the seat of language in the frontal convolutions; he has found disturbance of

speech much more common when the lesion was situated in the corpus striatum or optic thalamus than when it affected the anterior lobes.

In the organic or nutritive functions, disturbances are frequently found; vomiting is very common, and usually associated with headache; sometimes it occurs only in the mornings. The appetite is often lost, sometimes abnormally increased. Constipation is the rule, rarely diarrhea is seen. The pulse is slow during the early stages of the disease, but quickened towards the close. Respiration is slow; and, in children, often accompanied by a peculiar cry. Fever sometimes occurs early, but more commonly only at a late period, and is then significant of meningitis, or some other complication. In cases where the floor of the fourth ventricle is involved in the disease sugar would probably be present in the urine; it does not appear, however, as yet, to have been actually detected; but Schultzer has found inosite in the urine of two patients who suffered from cancer involving the fourth ventricle. Death, which usually occurs by coma and collapse, may be hastened by the exhaustion produced by vomiting and loss of sleep, or by cachexia, hectic fever, or bed sores. In many instances, however, the nutritive functions are long unaffected, and in cases of primary cancer of the brain the cancerous cachexia is very slow to appear.

From the above sketch it will be seen that there is no one symptom in itself diagnostic of a tumour in the brain, but that it is only by taking the whole of the symptoms presented by a patient, and by considering their successive appearance and grouping, that a tolerably certain opinion may be formed as to the nature of his disease.

In the second part of the work before us, we find a classification of intracranial growths based on the situation in which they are developed. A chapter is devoted to the symptoms produced by one or more tumours in each of the following localities:—1. Medulla oblongata; 2. Fourth ventricle; 3. Crus cerebelli; 4. Pineal gland; 5. Corpora quadrigemina; 6. Cerebellum; 7. Pons varolii; 8. Crus cerebri; 9. Pituitary gland; 10. Middle fossa of skull; 11. Corpus striatum, and optic thalamus. 12. Corpus callosum; 13. Middle cerebral lobes; 14. Anterior cerebral lobes; 15. Posterior cerebral lobes; 16. All three lobes at once; 17. Convexity of brain; and 18. Several different parts of the brain at the same time.

Of tumours affecting the medulla oblongata nine cases are given, one of which is original. In these the chief symptoms were

referable to disturbed sensibility, *e.g.*, pain in the head and limbs, or anesthesia; convulsions were frequently present, and in three instances partial or complete paraplegia was noticed. In the author's case, that of a man aged 43, suffering from a cancerous tumour in the upper part of the medulla oblongata, a curious symptom was observed; whenever the upper or lower limb on one side was touched, the sensation was referred by the patient to the corresponding part of the opposite limb, and this, although he perfectly knew right from left, and extended the right or left hand according as he was bid. The other symptoms in this case were attacks of occipital headache, with giddiness and vomiting; staggering gait; paresis of the lower extremities; tonic cramps in the arms; convulsive attacks, followed by loss of consciousness; amaurosis; dulness of intellect; hallucinations; and, towards the close of life, delirium, paralysis of the bladder, constipation, and finally, difficult deglutition.

The number of cases of tumours seated in the fourth ventricle, crus cerebelli, pineal gland, and corpora quadrigemina, is very small, and the symptoms are very imperfectly known. Two cases of tumour in the crus cerebelli are given; in each the disease was on the left side, and in each hemiplegia was present; but in one it affected the right side, and in the other the left. Our readers will remember Brown-Séquard's explanation of the paralysis of the side corresponding to the lesion, a paralysis "due not to any immediate alteration of voluntary motor conductors, but to an irritation of some parts of a crus cerebelli, producing a paralysis by a reflex action upon some other part of the nervous centres."^a

In two cases of extensive disease of the corpora quadrigemina occurring in children vision is said to have been unaffected. This is surprising, knowing as we do the close connexion there is between the optic tracts and the corpora quadrigemina. In neither case is any mention made of an ophthalmoscopic examination, so that it is very probable that, as Dr. Hughlings Jackson^b shows to be far from unusual in such cases, serious lesions may have existed unsuspected in the fundus of one or both eyes.

Of tumours of the cerebellum twenty-seven cases are given, one of which is original. The symptoms were, occipital headache, convulsive attacks, difficulty in standing or walking from deficiency of co-ordinating power, convergent strabismus, and amblyopia, or

^a Lects. on Phys. and Path. of the Central Nervous System. P. 201 and 263.

^b Ophthalmic Review. Vol. iii., p. 45.

amaurosis. Among the negative symptoms, conspicuous by their absence, were all disturbances of sensation, except headache, paralysis, lesions of speech, and mental derangement. Vomiting, which has been considered, by many, a symptom of great importance in cerebellar disease, was found in only twenty-six cases. The author does not look on it as in any degree characteristic of disease in this organ. He thinks there is no evidence to show that the cerebellum is the nervous centre of either the nutritive or the generative functions. Cases of cerebellar tumour generally terminate by extension of the disease, so as to involve the medulla oblongata, and interfere with deglutition or respiration.

Of tumours of the pons varolii twenty-six cases (one original) are recorded. The most characteristic symptoms were crossed paralysis, the face being affected on one side—that of the lesion, and the limbs on the opposite side; pain or anesthesia in the paralysed parts, various disturbances of the special senses, impairment of the mental faculties, difficulty of speech, and early appearance of dysphagia. Contrary to the opinion of Albers, the author finds convulsions to be a very rare symptom in tumours of the pons—so much so, that he lays down the following rule:—"If a tumour has attained sufficient size to allow of its presence being diagnosed, and if convulsions be present the probability is, that the seat of the tumour is not in the pons varolii."—P. 119. He, however, admits that in the very early stages of the development of a tumour the irritation caused by its presence may give rise to convulsions, while later the irritation of the fibres is followed by their destruction, and the convulsive attacks give place to paralysis. Disturbances of the mental functions are found in one half of the cases of tumours in the pons, as frequently in fact, as when the disease is situate in the cerebral hemispheres. This is very strange, and in the present state of physiology inexplicable.

Not less curious are the cases in which, although nearly the entire pons or medulla oblongata is involved in a morbid growth, yet neither paralysis nor anesthesia is observed in the lower limbs, but only uncertainty and irregularity in their movements. In explanation of this anomaly three hypotheses have been proposed:—1. That the nerve fibres of the pons and medulla oblongata are not necessary channels of communication between the spinal cord and the parts of the encephalon anterior to the pons. This view is, we believe, not held by any physiologist at present. 2. That the morbid growth does not destroy the nerve fibres, but merely separates them from

one another, and, by pressure, renders them thinner. This opinion is held by Funke; and the author thinks it best adapted to explain the phenomena in his cases. 3. The hypothesis proposed by Brown-Séquard, "that the divided extremities of the destroyed fibres become connected with either the tails or the envelope of some caudate or bipolar cells, and that by means of the fibres originating from these cells communication may continue from below upwards and from above downwards; if such a reunion takes place between the extremities of divided fibres, below and above the seat of the compression or of the alteration, although there is no decided paralysis, there must be much inaccuracy in the action of the will on the muscles, because certain muscles must be put in action at a time when the will does not wish for their contraction. As to sensations, if their intensity is not diminished, there must be a diminution in the faculty of determining from where an impression comes." * In tumours of the pons the difficulty of speech is manifested by stammering and imperfect articulation, and is due, in the opinion of the author, to implication of the roots of the hypoglossal nerve.—P. 123.

Seven cases of tumours, seated in the crura cerebri, are recorded. The chief symptoms noted are, lesions of sensation (pain, formication, or anesthesia) and paralysis of the limbs and face on the side opposite to the tumour—giddiness, and, above all, paralysis of the oculo-motor nerve, first on the side of the disease, and subsequently, as the tumour enlarges, on the opposite side; jerking and convulsions in the paralysed limbs were noticed in four cases. The author considers at some length the question, whether the sensory fibres contained in the crus cerebri come from the opposite or from the same side of the body, and concludes, in opposition to Schiff, that they are always crossed. Those few cases in which the disturbances of sensation are said to have been on the same side as the brain lesion, having been either imperfectly recorded, or having been complicated by disease in other parts than one crus cerebri.

Nine cases are given of tumours in the pituitary gland and parts adjacent; one of these is original. The symptoms briefly are, severe frontal headache, and amblyopia, or amaurosis, affecting first one eye and subsequently extending to the other; there are no very marked disturbances of sensation or motion—no difficulty of speech, and the mental powers are well preserved. In the author's case atrophy

* Brown-Séquard. *Loc. cit.*—P. 232.

of the optic nerves was discovered before death by means of the ophthalmoscope, and after death the retina was found, on microscopic examination, to be perfectly normal. Bader records a somewhat similar case. These observations agree very well with the results recently obtained, experimentally, by Dr. Rosow, of St. Petersburg, who found, in rabbits, thirty-nine days after section of the optic nerve, that the retina was scarcely altered from its healthy condition.^a

Two kinds of atrophy of the optic nerve are observed to follow brain diseases:—First, progressive atrophy, which is not preceded by inflammation, and is characterized by the uniform white colour and sharply-marked border of the optic disc, whereby it contrasts strongly with the adjacent parts of the fundus; when this condition has existed for any length of time atrophy of the nerve fibres and of the layer of ganglionic cells of the retina takes place; but the other layers are well preserved. The other kind of atrophy is that which follows neuritis—it is the most common; in it the edges of the papillæ are ill-defined, and the colour is less brilliantly-white than in progressive atrophy; the retinal vessels, though small, are tortuous, and pigment is frequently accumulated in the neighbourhood of the nerve entrance; here extensive retinal changes, due chiefly to hypertrophy of connective tissue and sclerosis, or fatty degeneration of the proper nervous elements, are found in the vicinity of the optic disc, and often in the region of the macula lutea.

It does not seem very clear as yet what determines, in a given case, a particular kind of atrophy. M. Wecker^b supposes that when an intra-cranial tumour grows very rapidly it causes neuritis, whereas, when the growth is more gradual, it may give rise to progressive atrophy, especially when it directly presses on the roots of the optic nerve. But it is not necessary that the intra-cranial disease should involve the optic nerve-fibres in any part of their course in order to produce amaurosis. Dr. Hughlings Jackson says:—"So far as the production of optic neuritis, by intra-cranial disease, is concerned, the position of the disease *seems* to be of little consequence, and there is nothing very peculiar in its nature, except that it is usually coarse."^c

This writer has, however, found optic neuritis to coexist much

^a British and Foreign Medico-chirurgical Review. April, 1866. P. 526.

^b Etudes Ophthalmologiques. T. ii., p. 386. Note.

^c Ophthalmic Review. Vol. iii., p. 48.

more frequently with hemiplegia on the left than on the right side; ^a and has never yet seen amaurosis associated with disease limited to the motor tract.^b In the case recorded by our author the form of atrophy which was present is not stated. In Bader's case^c the optic nerves and retina were very much congested, but there does not appear to have been neuritis; the tumour pressed on the pons varolii and upper part of the medulla oblongata, and compressed the right vertebral artery. In the amaurosis which accompanies loco-motor ataxy Jackson^d has found the optic discs to present the characters of white atrophy, and to show no sign of pre-existing neuritis; and Liebreich believes a greyish-blue colour of the discs to be almost characteristic of spinal amaurosis.^e Finally, Galezowski^f mentions a case of suspected tumour at the base of the skull, where one optic nerve underwent progressive atrophy, and the other was, at a later period, attacked by neuritis.

Returning from this digression, which we hope our readers will excuse in consideration of the interest which is at present being excited by ophthalmoscopy in nervous disorders, we come to notice briefly the symptoms presented by the remaining classes of tumours.

When the morbid growth is situated in the middle fossa of the skull the fifth nerve is constantly involved, as evidenced by pain, anesthesia, or inflammation in the parts to which it is distributed. The other symptoms of these tumours are not very characteristic of their position.

In fifteen cases where the corpus striatum or optic thalamus was the seat of disease the principal symptoms were hemiplegia, partial or complete, on the opposite side, and convulsive attacks; ordinary sensation, and the special senses were seldom affected, while mental impairment and difficulty or loss of speech were common. In considering this last symptom the author appears not to distinguish the speechlessness due to mental defect from that caused by inability to articulate; hence, his arguments against those who place the seat of language in the anterior lobes are not of much value.

Four cases of tumour in the corpus callosum offer no remarkable symptom which could distinguish them from those situated in the

^a Ophthalmic Hospital Reports. Vol. v., p. 56.

^b Ophthalmic Review. Vol. iii., p. 44.

^c Guy's Hospital Reports. 3rd series, Vol. viii., p. 64.

^d Lancet, 1865. Vol. i., p. 618—Vol. ii., p. 247.

^e Atlas, p. 35.

^f Etudes Ophthalmoscopiques sur les altérations du nerf optique, p. 87.

cerebral lobes. Twenty-seven cases of tumours of the middle cerebral lobes are given, and twenty-seven where the new growth was seated in the anterior lobes. In each series we find hemiplegia, convulsions, and mental disturbances to occur in about equal proportion. Lesions of sensation are more common in tumours of the middle lobes, and chiefly show themselves as anesthesia of the paralysed parts. Impairment of the sense of smell is common when the anterior lobes are the seat of disease; the other special senses are more frequently affected when the tumour is situated in one of the middle lobes. Difficulty of speech seldom accompanies tumour of the anterior lobes, and while headache is common, it is seldom confined to the frontal region.

In tumours of the posterior cerebral lobes (fourteen cases) headache, though usually present, was not referred to the occiput, but was, in general, diffused over a great part of the head. The organs of special sense were very rarely affected. Paralysis was either slight or altogether absent; convulsive attacks were common, and mental disturbances, particularly such as indicate depression, were frequently observed. Nineteen cases where all three lobes were together occupied by one or more tumours show that, while headache is common, other disturbances of sensibility are rarely met with. Hemiplegia is not very common, but epileptic attacks are by no means infrequent. The organs of special sense do not suffer much. The mind is often impaired.

Seventeen cases of tumours of the convex surface of the brain presented the following symptoms:—Headache, generally limited to one region of the head, but not always to the seat of the tumour; epileptic convulsions, and mental disturbances, generally of an irritative kind—as mania, monomania of riches, &c. Anesthesia, paralysis, and lesions of the special senses were, as a rule, not observed.

The last chapter treats of those cases where several tumours are found situated in different parts of the brain; fifty-two examples of these are given. As may be supposed the diagnosis is, in most cases, impossible, the symptoms of the larger tumour generally masking those which are produced by the smaller growths. The difficulties are fully recognised by the author, and those cases in which some certainty of diagnosis is attainable are distinguished from those where the true nature of the disease can only be revealed *post mortem*.

In conclusion, we would recommend to our readers the work before us as a valuable contribution to the literature of cerebral disease.

Codex Medicamentarius, Pharmacopée Française. Rédigée par ordre du Gouvernement, la Commission de rédaction étant composée de Professeurs de la Faculté de Médecine et de l'École Supérieure de Pharmacie de Paris, de Membres de l'Académie Impériale de Médecine et de la Société de Pharmacie de Paris. Paris: J. B. Baillière et Fils, 1866.

Codex Medicamentarius: French Pharmacopœia. Compiled by order of the Government, the Committee of Publication being composed of Professors in the Faculty of Medicine and of the Upper School of Pharmacy of Paris; of Members of the Imperial Academy of Medicine and of the Pharmaceutical Society of Paris. 8vo, pp. 784.

THE new French *Pharmacopœia*, which has just appeared under the sanction of the Imperial Government, would seem to have been rather more than five years in preparation, the necessary authorization having received the Emperor's signature at the Palace of Fontainebleau on the 20th June, 1861. The dates of the former editions have been respectively 1748, 1818, and 1837.

"The *Codex Medicamentarius* (we are informed in the Preface) is composed of three divisions:—Preliminary Information; *Materia Medica*; *Pharmacopœia*. Under the title of Preliminary Information (*Notions Préliminaires*) the *Codex Medicamentarius* comprises the most general and most usual numerical data as to the correspondence of the ancient French weights and of foreign weights with the metrical system; the estimation in weight of spoonfuls, drops, pinches, handfuls, &c.; of the liquid or solid substances often prescribed under these practical forms; tables of density relating to the liquids in frequent use; the indications necessary for the choice and use of areometers and the densimeter; the points of fusion, the boiling points, the solubility of substances in common use. A table of the equivalents of the simple bodies in use in medicine, whether by themselves or in their combinations, terminates this first part."

The great utility of the preliminary information given in the *Codex* is evident from the foregoing enumeration.

The substances constituting the *Materia Medica* are arranged in alphabetical order. Of the more important, as opium and cinchona, copious descriptions are given, extending in the case of the former to a page, and in that of the latter to three and a half pages of this division of the work, which latter is subdivided into two sections,

one containing substances obtained directly from vegetables or animals, the other those derived from minerals and chemical products.

In the *Pharmacopœia*, properly so called, on the contrary, the methodical order has been followed:—

“The reasons for this decision need scarcely be specified. We find on each page, in fact, preparations advantageously brought together, because their nature admits of it, though they differ in name; an alphabetical arrangement would have separated them, thus requiring the useless repetition of certain details, or obliging the pharmacien to search in three or four distinct and distant places in the *Codex* for information indispensable to the execution of a single formula.”

The *Pharmacopœia* again includes two classes of products, one comprising simple medicines supplied by chemistry, the other medicines produced by the mixture of different matters.

Fourteen chapters are devoted to simple bodies, acids, oxides, alkalies, ammonia, chlorides, &c., and mineral salts; eight to acids, &c.; one to artificial mineral waters; fifty-one to various Galenical preparations, &c.; while the seventy-fifth and last chapter contains formulæ in common use abroad, little or not at all employed in France.

Lastly, towards the end of the work, we have extracts from the laws and regulations relating to the practice of pharmacy; laws and ordinances bearing on the sale of poisons; decree relating to secret remedies; royal ordinance respecting the manufacture and sale of artificial mineral waters and of gaseous waters.

Having thus described the general arrangement of the volume, we shall next direct attention to such of the preparations as may strike us, in running rapidly through its pages, as presenting some peculiarity. A remarkable feature is the immense number of syrups, the enumeration of which, in the French index, occupies three columns. It seems scarcely necessary to give directions for the preparation of “pulvis sacchari.” Cherry juice, gooseberry juice, raspberry juice, oil of eggs, appear to be rather out of place in a pharmacopœia. We have, of course, an extensive chapter of “ptisans,” to which are appended four lemonades and a formula for *petit-lait*. Our suspicion that the well-known culinary talents of the French were predominating culminated on reaching Chapter XXX., for though we are told that it is to be restricted to soups destined to fulfil a medical indication, we find that the

category embraces veal broth (two varieties, meat and light) chicken broth, frog! and turtle soup!! and—defend us from such physic, or dietetics, whichever it may be—snail broth. To the English mind it certainly appears strange to see such matters occupying the serious consideration of men like Dumas, Rayer, Bouchardat, Grisolle, Regnaud, Tardieu, Wurtz, Bussy, Chatin, Guibourt, Le Canu, Buignet, Goble, Mayet, Mialhe, Schaeuffele, Petit, and Mourier, many of whose names are well known in science. The vulnerary tincture, which is dignified with an asterisk, the mark of such preparations and drugs as are to be kept in all pharmacies, contains nineteen ingredients. We have a formula for “English Black Drop,” a chapter of ethereal tinctures, a tranquillizing balm also with nineteen ingredients, a useful table of the quantities of extract produced by a given weight of each of a large number of substances therein named; a syrup of snails! which we are told may be made either with the *Helix aspersa* or with the *Limaçons des vignes*; a *syrupus de pulmonibus vitulinis compositus*, and a *syrupus de Artemisia compositus*, truly compound, containing eighteen ingredients. Of chocolates we have seven, including vanilla, sago, arrow-root, tapioca, Iceland moss, ferruginous, and *chocolat de santé*. A chapter of very complex electuaries is crowned with the famous Theriaca, with its fifty-nine ingredients, including dried vipers! And this close, as we now are, upon the last third of the nineteenth century. Next come gelées, pates, oleosaccharures, saccharures, tablettes, and pastilles, most of them very sweet and good, and many of them, no doubt, useful. We have *species amaræ*, anthelminticæ, aromaticæ, astringentes, pectorales, carminativæ, thea helvetica (twenty ingredients), and fructus pectorales; tooth powders, sternutatory powder, and *pulvis ad condienda cadavera*; granules of digitaline, arsenious acid, atropia, and strychnia; capsules, “cold cream,” and cucumber pomade; papiers emplastiques, medicated baths and pediluvia; glycérés, escharotics, fumigations, and cigarettes.

On the whole, we are disappointed in the *Codex Medicamentarius* of 1866. We regret to see the culinary element so predominant, though this was perhaps to be expected, and is not without its utility. But we regret still more to see some of the absurdities of two or three hundred years ago retained in a work produced by a committee numbering among its members some of the most distinguished cultivators of pharmaceutical science of the present day. At the same time, the *Codex* will be found a most useful book of

reference, both on account of the numerous valuable tables it contains, and of the universal tendency its editors have sought to give it. In fact, in the present day, when, as they observe, international intercourse has so much increased, such a work is indispensable to the British pharmacien. Nevertheless, our conviction is, that the framers of the new edition of the *British Pharmacopœia* will see in the work very much most carefully to avoid, and but little to adopt.

Tetanus.—Eine Physiologische Studie. Von DR. JOHANNES RANKE, Privatdocent der Physiologie und Assistent am Physiologischen Institute in München. Leipzig: Verlag von Wilhelm Engelmann, 1865, pp. 468.

Tetanus.—A Physiological Study. By DR. JOHANNES RANKE, Lecturer on Physiology, and Assistant in the Physiological Institute in Munich. Leipzig: Engelmann, 1865, pp. 468.

(Continued from Vol. XLI., page 409).

IN the former part of this review we attempted to trace the progress of the subject up to the time when Dr. Ranke took it up. The author's own researches are divided into three sections:—

1st. On the influence of tetanus (*i. e.* muscular action) on the chemical composition of the muscles.

2nd. On the influence of tetanus on the general consumption of material in the living organism.

3rd. On the chemical arrangements which tend to control muscular action.

It would be quite impossible, however much we should desire it, or however much the importance of the subject may seem to warrant it, to enter at length, in a notice like the present, into the details of Dr. Ranke's investigations. We must be content to say that his methods of research seem to be extremely accurate, and that they are elaborate in the highest degree; many experiments have been undertaken to prove one single fact; a few pages, or even a single table, contain the result of the labour of many days; and although by far the greater part is quite original, yet nothing is omitted of what others have done on the same subject.

At the conclusion of each chapter a summary of results is given;

and we think that, even at the risk of making a bald and dry statement, we shall best give our readers an idea of what Dr. Ranke has accomplished by transcribing some of these results.

The animal tissues, and muscle among others, are, as is well known, very imperfect conductors of electricity. When muscular tissue, in consequence of its action (whether natural or artificial), becomes more or less impregnated with materials resulting from the disintegration or chemical change in its structure, resulting from contraction, its power of conducting electricity becomes notably increased. As a rough qualitative test, therefore, to prove that chemical change of some sort has taken place, the power of conducting electricity becomes one of great delicacy. In some pages, which constitute a brief appendix to his introduction, Dr. Ranke gives some historical observations upon the experiments up to the present time for the determination of the resistance offered by the animal tissues to the passage of the galvanic current. The first chapter of his own researches contain comparative determinations of the resistance offered to the galvanic current in living and dead muscle, and muscle in a state of repose, and which had been tetanized. Previous to Dr. Ranke's researches the only conclusions of any value arrived at were those of Du Bois-Reymond, viz., that the resistance offered by muscle is remarkably diminished by cooking; yet that albumen does not alter its power of resistance in consequence of coagulation.

In the first chapter of his first section the author speaks of the resistance which muscular tissue offers to the passage of the galvanic current in its relation to the chemistry of muscle and of tetanus.

Results are as follows:—

“1st. The loss of the vitality of muscle is found to be allied to a diminution of the resistance which it offers to the passage of galvanism.

“This in mammalia (rabbits) amounts to about one half, and in frogs two-thirds of the original resistance.

“Living muscle conducts about three million times worse than pure quicksilver, and 115 million times worse than pure copper.

“Dead muscle conducts about 1.68 million times worse than quicksilver, and 62 million times worse than copper.

“2nd. This diminution of resistance, which accompanies loss of vitality, depends upon an accumulation of those products, themselves good conductors, which result from the disintegration of the muscular tissue, which is itself a bad conductor, or does not conduct.

“The acid which makes its appearance in muscle at the time that it

loses its vitality plays an important part in the improvement which takes place in its conducting power.

“3rd. The diminished power of resistance offered by muscle which has been cooked, as Du Bois-Reymond found, is explicable in the same way; as is also the diminution of resistance which, according to Du Bois-Reymond’s discovery, accompanies muscular contraction.

“4th. The diminution of resistance which accompanies loss of vitality is much less in muscles which have previously been submitted to exhausting muscular contraction than in those which have been in a condition of repose. During *rigor mortis* the amount of disintegrated muscular tissue is notably diminished by tetanic spasms having been previously excited.

“5th. The diminution of resistance, during the loss of vitality in muscle, and the amount of disintegrated material in *rigor mortis* is liable to very remarkable individual variations.

“6th. As is also the quantity of the destructible material lost in consequence of tetanus.”

Although requiring a method of research quite different from the foregoing, yet the results of the second chapter, on the water contained in muscle, are hardly less interesting.

Results:—

“1st. Tetanus is always accompanied by a diminished per centage of the solid materials of muscle, with which there is a corresponding increase of the per centage of the water contained in this tissue.

“2nd. Slight muscular action is allied with slight increase of the water; action of muscle and increase of the water contained in its tissue are in exact relation.

“3rd. The less the quantity of water previously contained in muscle while in a condition of repose the greater is the increase of water as the result of tetanus.

“The greater the per centage of solid matter contained in muscle during repose so much the greater is its capability of doing work.

“4th. The per centage of the solid material of muscle increases in man along with the capability of performing work, from youth up to vigorous manhood; from this on to advanced age there is a corresponding diminution.

“The time when the water contained in muscle is at its minimum corresponds with the time when the muscle has the greatest capability of performing work.

“5th. After prolonged periods of insufficient nourishment we find that a diminution of the solid material of muscle goes hand in hand with a

corresponding diminution in the solid constituents of the central organs of the nervous system.

“ 6th. In special organs the muscles which are subject to most exertion are the richest in water. The muscular structure of the heart is the richest in water.”

These extracts are enough to show something of the general plan followed in the work, but nothing short of perusal of the book itself can give any accurate notice of the methods of research followed in obtaining these results.

We shall now briefly enumerate some of the practical conclusions arrived at by Dr. Ranke.

When removed from the body, muscles which have been tetanized show no perceptible increase of the water contained in them; the blood of the animal is, however, affected by tetanization of the muscles; it becomes poorer in water and richer in solid materials, in fact, we find in the blood the matters which muscles lose by tetanization. The water extractive matters are diminished in muscle which has been tetanized while the circulation has been still maintained in it; even, however, when the influence of circulation is excluded there is a tolerably marked diminution of the water extracts in muscle which has been tetanized. In this latter case both Helmholtz and Ranke have pointed out the important fact that the diminution of the water extracts corresponds with an equally great increase of the alcoholic extracts.

Muscle, after it has been removed from the influence of the circulation is found to possess a tendency to the formation of acid, and there appears to be a fixed and unalterable maximum of the amount of acid capable of being formed; this maximum is found to be lowered in consequence of the tetanization of the living animal. Muscle which has been tetanized produces less acid than muscle which has been at rest; in fact the tetanization of muscle seems to consume the acid-forming matter.

Valentin and others had observed that muscle, during tetanization, gives to the atmosphere more carbonic acid than when in a condition of repose; and Valentin further found that muscle, after the cessation of its irritability, gives off carbonic acid and takes up oxygen; to this Ranke has added the fact that a diminution in the amount of carbonic acid given off after the death of the muscle follows when the muscle has been previously tetanized.

Chapters VII. and VIII. are devoted to a comparative determination of the amount of sugar and of fat contained in muscle which has been in action through tetanization and in a condition of repose. The results will have a great interest for those who are engaged in the consideration of the pathological condition connected with diabetes and fatty degenerations.

Dr. Ranke confirms the statement of Meissner as to the occurrence in muscle of a true sugar capable of undergoing fermentation. He warns experimenters, however, that there exist in decoctions of muscle other matters besides sugar which are capable of reducing the oxide of copper of the cupro-potassic test solutions; kreatine, and especially kreatinine, possess this property to a certain degree.

Muscle, he finds, is richer in sugar after it has been tetanized than it was before when in a state of repose; and the increase of sugar resulting from tetanus is something very notable. The liver has no influence on the increase of sugar during tetanus. Sugar originates in the muscular substance itself during tetanic action of the muscle. The amount contained in the frog-muscle, during repose, amounts to 0.58 per mill.; that in muscle which has been tetanized to nearly 1 per mill. (0.93) of the dry muscular substance.

Five experiments agree in the result that tetanization of muscle causes an increase in the normal amount of fat present in the muscle; this fat must have been formed from the muscular structure itself.

The dry residue of muscles which have been at rest and of those which have been tetanized show no appreciable difference in their nitrogenous contents; these on an average amount to 14.4₀/°.

The quantity of albumen, however, in muscle is diminished by the action of tetanization; and this is true not only for the albumen which is soluble in water, but for the total quantity of it which the muscle contains.

The second section of Dr. Ranke's work, which treats of the influence of muscular action on the general consumption of material in the living organism, and the elimination of the material consumed, has a great interest for physiologists at the present time, when this obscure and difficult subject is attracting the notice of so many. Whether, according to the views of Lehmann, there is usually going on in the animal economy a "luxus consumption," *i. e.*, a conversion of albuminoid matter into urea in the blood before tissue-formation has taken place, or whether there be

sufficient potential energy in the nitrogenous tissues and in the oxygen required for their transformation to account for the dynamical actions of the living frame; or whether, in short, the excretion of urea is a true and precise expression of the amount of work done in the organism, are among the fundamental questions in physiology—questions upon the right answers to which great theories stand or fall. All observations, therefore, bearing upon the subject which can be relied upon as accurate have a great value.

Dr. Ranke's researches on this head embrace the following topics:—

1stly. Some of the relations existing between the blood and muscular work.

2ndly. On the daily elimination of carbon and of nitrogen from the human organism when sufficient nutriment is given, and the muscular system is in a condition of repose.

3rdly. On the daily elimination of carbon and nitrogen from the human organism under varying conditions of nourishment (want of food, meat diet, non-nitrogenous diet), combined with repose of the muscular system.

4thly. Of the eliminations which take from the animal organism under the influence of muscular work.

The results of the second and third chapters of this section of his work Dr. Ranke has already published in Reichert's and Du Bois-Reymond's *Archiv.* for 1861, p. 311. His experiments have been conducted with the aid of that elaborate apparatus which has made Professors Bischoff, Voit, and Pettenkofer so famous, while the experiments detailed in the last chapter of this section have been carried on in the laboratory of Professor Liebig.

Dr. Ranke finds that it is possible, with a certain alimentation (under conditions already established for the carnivora by Bischoff and Voit), so to regulate the elimination of nitrogen from the system, that not less nitrogen shall be carried away from the system in the excreta (urine and feces) in twenty-four hours than is ingested during the same period in the food. He has proved, with regard to man, the proposition already demonstrated for the carnivora by Bischoff and Voit, that equilibrium of the nitrogenous income and expenditure first takes place in man when the consumption, not only of nitrogen but of carbon also, during the time of experiment is completely covered. With the proportions of carbon and nitrogen in the food the excretion of nitrogen varies so within

certain limits, that with a surplus supply of carbon the excretion of nitrogen diminishes. The direct estimation of carbon in Pettenkofer's apparatus gave for one day of experiment (during which, according to Dr. Ranke, equilibrium between the supply and expenditure of nitrogen had occurred) the weight of 207 grammes of carbon as the amount eliminated in the expired air in twenty-four hours. It does not appear that any constant proportion of nitrogen and carbon in the food is required for the attainment of a state of equilibrium between the food and excreta.

As regards the effects found to result in man from varieties of diet, want of food, &c., the facts observed harmonize very closely with those already discovered by Professors Bischoff and Voit in the case of their well-known "*hund*," which has done such good service for science.

The third section of Dr. Ranke's book contains a great number of very beautiful experiments, which are, so far as we know, quite original, and are certainly in the highest degree interesting. That the capability which living muscle has of exercising certain functions may be maintained by the injection of oxygenated blood through the vessels after the part has been removed from the body is well known. Blood, however, is a fluid containing so many ingredients that it is impossible to surmise which of its many components may be the more active in maintaining this capability of action in muscle tissue. Dr. Ranke's experiments consist in injecting through the vessels of muscles less compound fluids, and investigating the condition of the muscle as regards its capability of contracting. In this way he has investigated the effects on muscle of lactic acid, kreatine, kreatinine, sugar, and carbonic acid. He has obtained the following results:—

"The capability which muscle has of exercising its functions depends for its strength upon the presence or absence of certain matters formed in the muscle itself by the decomposition of the muscular tissue.

"The greater the quantity of the accumulated products of decomposition in the muscle the less is the capability of the latter for discharging its functions.

"The sugar, however, which accumulates in tetanized muscle has no exhausting effect upon the muscle, nor does sugar exercise an influence upon the system at large.

"Indirectly sugar may have some effect, by giving rise to the development of lactic acid.

"Kreatinine and carbonic acid are circumstanced very similarly to

sugar. Both substances have rather slight influence upon the muscular tissue, and the peripheral nervous system connected therewith.

“Both somewhat depress the vital properties of muscle and of the peripheral nerves, but only in a very unimportant manner.

“As to kreatinine it is probable that instead of an exhausting influence, lessening the capability of the muscle, it maintains or restores the latter by neutralizing the lactic acid accumulated in the muscle.

“The action of the carbonic acid, which is so slight on the muscle, and also on the peripheral nervous system, is proportionally more violent on the nervous central organs in the spinal cord and brain. The final effect of the accumulation of carbonic acid in the system is a paralysis of the nervous central organs.

“Quite different are the results of investigation with respect to *lactic acid* and *kreatine*. These are to the muscle exhausting matters. At the same time we observe that they act in an opposite manner on the peripheral nervous system; they elevate the excitability of the muscular nerves.

“An additional fact, experimentally proved with respect to the action of kreatine and lactic acid, is, that both the substances in question are muscular stimulants. The muscle contracts when it is brought into contact with these bodies.

“The idea suggests itself that having ascertained that the products of decomposition of the muscular substance—lactic acid and kreatine—are muscular stimulants, we are brought nearer to the solution of the problem of the proximate cause of the production of muscular contraction.

“Muscle, under the influence of the nervous system, itself produces, by the decomposition of its own substance, matters which cause it to contract.

“The simple removal of the exhausting matters from the muscle restores the normal vital properties of the latter. If lactic acid alone has acted as the exhausting substance, neutralisation of the acid by carbonate of soda produces the same effect.”

In concluding this very imperfect notice of a work which, as we have already said, places its author in the foremost rank among physiologists, we may be permitted to transcribe, *in extenso*, the final chapter. We earnestly recommend the book to our readers, as it is only by a careful perusal of its details that one can judge of the accuracy of the conclusions arrived at; the chapter, however, which we here transcribe gives in a short space an epitome of the views and generalizations of the author, and shows not only the object of the work, but how far that object has been attained:—

"In conclusion, we have to take a rapid glance at the course and results of our investigation.

"The reader will recollect that we commenced the experimental investigation in question under the impression that the well-authenticated results of inquiry as to the influence of tetanus on the metamorphoses of matter of the system hitherto obtained, and at the same time the views thence derived as to the production of muscular power, led to an apparently irreconcilable discrepancy.

"On the one hand it was established by the researches of the most distinguished investigators, as Liebig, Helmholtz, Du Bois-Reymond, and others, that tetanus of muscle was connected with certain demonstrable changes of its substance, all indicative of a consumption of muscular matters by muscular action, and thereby leading necessarily to the inference, that the source of the muscular power was to be sought in that consumption, in that decomposition or oxidation of the muscular matters, and therefore in the force thereby rendered free and available.

"On the other hand the remarkable investigations of Th. L. W. Bischoff and C. Voit upon the nutrition of the carnivora had decidedly and incontestably demonstrated that the conditions of metamorphosis of tissue, the oxidation of matter of the animal organism, are dependent, not on premises to be arbitrarily assigned, but simply on the amount of the nutritious material laid up in the body, and its qualitative composition. To this result of the investigation the experiments of C. Voit had further afforded the certain and direct proof, that the excretion of urea could not be increased by muscular work.

"From these latter researches it follows, with all possible certainty, that in the material laid up in the system the decompositions cannot be arbitrarily increased, but that they take place only in consequence of internal causes.

"The oxidations and metamorphoses of matter which we see proceeding in the organism, and in which alone we can seek the source of the development of power in the animal system, are independent of the use of these mechanical powers in functions without the system, they are not increased thereby.

"We see at the first glance that these contradictory results, which on the one hand clearly demonstrate a consumption of matter through muscular work and tetanus, but on the other prove the independence of the aggregate changes in the system for the production of muscular work, may perhaps be explained by assuming, that in considering a longer period of time, as was the case in the above-mentioned experiments of metamorphosis of matter, such an independence may exhibit itself, but only because the decomposition of matter, increased during muscular action, follows directly a diminution in its energy corresponding to the increase. As science had accustomed itself to consider the

ological Study.

excretions of the system as a measure of the metamorphoses of it, the question appeared to be: may we not perhaps, notwithstanding on the whole the excretion of nitrogen from the body is not in the production of muscular work, in considering as short possible, demonstrate an increase of the excretions corresponding production of energy.

"If following upon such a supposed increase in the bodily corresponding diminution were demonstrable, an accordance results in a simple manner would thus have become possible. On one hand the origin of muscular power were demonstrated in a mechanical manner, on the other the second proposition would be illustrated, that the magnitude of the metamorphoses of matter, of oxidations, and, therefore, also the magnitude of the power, in a longer period, is developed by the organism, are independent of the external work performed during this period.

"We entered on the investigation. It was easy, by direct examination of muscle, partly to confirm the old observations of the composition of matter in muscle; indeed we succeeded in discovering a tolerable number of new facts, explicable solely by this consumption. We, moreover, found that with the increase of the consumption of muscular matter in a sequence of tetanus a number of facts pointed to a change in the composition of the chemical decompositions in muscle during the period of increase. We also observed that the metamorphoses of matter during tetanus took place on which the development of the muscular power depends, only within the muscle itself, so that we are constrained to consider the muscle not only as a conducting medium of a force arising within it, but also as the substance oxidised and metamorphosed in the production of the work.

"Thus one side of the problem, the proof that muscular power depends on the forces of the muscular substances set free and rendered available by increased metamorphosis of matter, was satisfactorily established. Even though we are not yet in a position to measure these forces directly, and rendered available in tetanus or muscular work by the decomposition of the matters of the muscle, and quantitatively to determine their accordance with the amount of work produced, the demonstration is sufficient, that an increase of the forces set free takes place, which may be looked upon as the basis of the production of work.

"We were thus enabled to turn to the second part of our problem, the solution of the apparent contradiction resulting from the determination of the excretion of urea in muscular action.

"The problem, as we had above propounded it, seemed to have a simple solution. It is true that for this purpose we had, as the conditions seemed to apply only to man, to make a tedious and 1

preliminary investigation, in order to establish the fact that results obtained in man are comparable with those obtained in the carnivora. The demonstration proved satisfactory; we had recourse to the particular experiments, which should, in considering shorter periods of excretion, establish the existence of an increase of the excretion of nitrogen in the urine proportional to the production of work. How great was our amazement to find that, notwithstanding the increased metamorphosis of muscle in tetanus, no such corresponding increase was demonstrable in the nitrogenous excretions in the urine. There were certainly changes in the excretion of urea in consequence of tetanus, but these were, notwithstanding the similarity of the work done, not comparable with one another. The same result, which was demonstrated by C. Voit in so striking a manner for longer periods of investigation, was likewise obtained, though with some modifications, in considering very short periods of time.

“It thence appeared strikingly, that the excretion of urea is not capable of affording a faithful representation of the slight variations of the metamorphosis of matter in the organism, that in it only the greater features of the same are exhibited; for manifestly actually existing synonymous changes of metamorphosis of tissue were not in a position to yield an equivalent result in reference to this excretion.

“There was an *irregular* slight increase of the excretion of urea, upon which a period of diminution supervened.

“Estimations of the increase of the yield of carbonic acid occurring under the influence of muscular work were also undertaken. They afforded a result similar to that of the determination of the urea. In the increase of carbonic acid also it is impossible to discover a direct connexion between it and the muscular work produced. Indeed in the excretion of carbonic acid during work as well as in that of urea, a diminution may take place instead of the expected increase which diminution in the one case as in the other is followed, in the period of rest subsequent to the production of work, by an increase lasting for a period of variable duration.

“For the excretion of carbonic acid also C. Voit's observation on that of urea seems to show, that in the consideration of longer periods the excretion is not increased by muscular work, that therefore on the whole the processes of decomposition and oxidation, on which this excretion depends, are independent of and uninfluenced by the work performed. Still these periods appear to be longer than they showed themselves to be with reference to the excretion of urea.

“Thus, then, my experiments, as well as the certainly much more comprehensive ones of Bischoff and Voit, prove that the magnitude of the metamorphosis of matter, and at the same time the amount of power set free and rendered available in the animal system during a longer

bitrary influences, as, for example, as we have before us a function of independent of external influences. Indeed, my experiments showed even with respect to the excretion of urea, that tolerably short periods of time, as an hour, are sufficient almost completely to remove an existing change in their results.

"No other solution remained to us then, than the assumption, that in *very short* spaces of time a compensating process must take place within the muscle, whereby the changes induced by muscular action in the conditions of metamorphosis may be almost completely counter-balanced. But how are we to imagine this compensating process? Wherein are we to find the basis of a limitation of the decompositions in working muscle?

"On the one hand we find an increase, on the other a diminution of the muscular decompositions, the one during, the other after the production of work in the muscles. It is evident that both processes—the increase and the diminution—appear equally inexplicable.

"I was so fortunate as almost completely to solve this enigma. We found in the occurrence of lactic acid and kreatine in the muscle the foundation of both apparently irreconcilable facts.

"These substances are not only very powerful muscular stimulants, and we may well suppose that the occurrence of muscular action is due primarily to their stimulating operation; they also elevate the nervous excitability, so that under their primary action an impulse proceeding from the nervous central organs must produce augmented developments in the nerves. But while they primarily and in smaller quantities are capable of increasing the operations of the muscles and nerves, they subsequently gradually diminish the same by increased accumulation, until they have at last completely annihilated them. Thus this observation gave us reason to show, how with and in consequence of muscular action, which notoriously proceeds with accumulation of the two matters mentioned, a stage of increase of decompositions—elevated capability of action—may be followed by a stage of diminution—fatigue, incapability of action.

"I further succeeded in demonstrating that the chemical bodies mentioned produce no definitive alteration of the muscle by their presence, but that their effects may be again destroyed by their simple removal from the muscular juice by washing out and neutralisation of the acid. It was obvious that we had thus discovered the cause of the restoration of the fatigued muscles—lactic acid and creatin are partly washed out of the muscle by blood and lymph, partly are they rendered uninjurious in consequence of the neutralisation of the lactic acid by these alkaline fluids.

"But the muscles, free from blood and lymph, exhibit also outside the system a power of recovery. Sarokow's observation, that in tetanus the

strongly basic kreatinine is formed from kreatine, was capable of explaining this phenomenon also. Kreatinine is almost without action on muscles and nerves. When therefore a part of the kreatine is changed into kreatinine, the newly formed kreatinine in virtue of its basic properties, serves to neutralise the acid. Direct experiment confirmed this *à priori* assumption.

"We had thus discovered the means of restraint; but after this important advance the further problem remained: to investigate the proper ground of their action.

"We succeeded in showing that the electro-motor power of the muscle is arrested under the influence of the fatiguing matters, that with the greater accumulation of lactic acid and kreatine it disappears, and that it may be reproduced by the subsequent removal of these principles. As we are constrained to consider the mode of occurrence of the oxidation of the muscle as influenced by the strength of the electrical currents circulating in it, this observation gave us indications as to the mode in which the fatiguing substances may produce a change in the extent and direction of the oxidations and decompositions in the muscle.

"Experiments instituted according to Schönbein's process on the action of the fatiguing matters on superoxide of hydrogen, threw still more direct light upon this action. We observed that present in small quantities, they are capable of forming an excess of ozone, but that in more considerable quantities they employ all the ozone formed in another way, in their own decomposition (?). These experiments show clearly how they, present in smaller quantities in muscle, may primarily increase its oxidations, as they prepare active oxygen for it, but secondarily in larger quantities, by withdrawing all the ozone formed from it, are capable of wholly annihilating the oxidations.

"The observations on the action of the fatiguing matters on the fermentation of saccharine fluids are in perfect accordance with these results just mentioned.

"I thus close the present investigation. I have succeeded in solving, as I hope satisfactorily, apparent experimental contradictions. *Notwithstanding that on the whole the bulk of the oxidations and decompositions in the animal organism owe their occurrence exclusively to the decomposable amount of matter at the same time present therein, the forces freely expended in muscular work are supplied by a momentarily increased oxidation or decomposition taking place within the muscle, whose apparently necessary effects on an increase of the excretions of the system are counteracted by recognized, and in their actions, intelligible restrictive arrangements.*"

Surgical Experiences:—The Substance of Clinical Lectures. By SAMUEL SOLLY, F.R.S. London: R. Hardwicke. 1865. 8vo., pp. 656.

PERHAPS that which we have most to deplore, on reading of the death of any of our great medical or surgical authorities, is the conviction of the vast mine of *experience* that on all such occasions must have been lost to the public and our profession. No matter how industrious or how accurate in revealing his observations, an unknown quantity of invaluable experience disappears from the service of suffering humanity on the occasion of the death of such a man; a quantity that has to be laboriously reaccumulated by his successor only to be again lost on his departure from amongst his fellow-labourers. This is one of the serious obstacles to the continued advancement of our profession as a science; to a certain point it may be advanced by each successive labourer, but to leave to his successor as a legacy his accumulated wealth of, as it were, intuitive experience is placed beyond the reach of poor finite humanity; and in this way our profession seems to have allotted to it a task in some respects resembling that of Sisyphus. How much more precious than jewels, money, or lands, would be the legacy to a laborious, well-educated, intelligent surgeon, on the part of such a man as Brodie or Crampton, of the experience derived from a long life's accurate observation of disease in all its Protean forms! How impracticable it is for such men to give expression, either in words or in writing, to the intuitive perceptions which such long-continued experience has eventually yielded them! and how grateful should we not feel to those master minds in our profession, who, sacrificing all considerations of self-indulgence, laboriously toil to place on record, for our instruction, the results of their experience derived from years of patient and long-continued observations of the many and intricate medical problems that have been submitted for their consideration and solution! Such a work is the present emanation from the pen of the distinguished senior surgeon of St. Thomas's Hospital, a gentleman who, years since, had succeeded in inscribing his name honourably amongst those whose literary labours had placed our profession under obligations to them by his classical monograph on the *Anatomy of the Brain*.

Mr. Solly's work is composed of a series of lectures, fifty-four in

number, in which are recorded with more or less detail, his practical experience on some of the most important medical and surgical affections of the human frame. Amongst these may be specially noticed the lectures on Diseases of the Spine—Injuries of the Head—Injuries of the Head and Spinal Cord—Scrivener's Palsy—Diseases and Injuries of Joints—Lithotomy—Stricture—Hernia. Mr. Solly by no means pretends that the reader will find in his book exhaustive treatises upon all, or indeed, any one of these subjects; quite the contrary; but he will find what is far more valuable to the busy practitioner, shrewd practical remarks upon many of the most important subjects in medicine and surgery, illustrated with cases bearing upon the subject discussed, in the treatment of which can be easily recognized a master's hand. These are Mr. Solly's own observations upon this subject:—

“I here bring my labours for the present to a close. The form in which I have presented them to the public, but more especially to my friends and former pupils, will, I hope, recall the happy days of hospital pupilage. As clinical lectures, they profess to relate to nothing but that which concerns bedside practice, and the immediate application of the leading principles of surgery. They are, therefore, imperfect, because they do not go into all those details which belong exclusively to the regular course of lectures delivered in the schools. On this account it must be taken for granted that they are chiefly addressed to such as are already in some degree experienced in their profession, and that the beginner, while he cannot fail to gain an insight into the nature of the subjects he will necessarily have to handle, will, nevertheless, discover that he has yet much to learn before he can enter into the spirit and fully appreciate the value of the lessons he might otherwise gather from them. Those who are already engaged in the turmoil of practice will, I believe, agree with me, that it is pleasant in after years to refresh our memories by going back to the events in which we took our parts; to recover something from the lapse of time; to compare the present with the past; to live over again more than a quarter of a century—*grande mortalis ævi spatium*; and, in fine, to communicate to others what we know and think, before the promptings of ambition have ceased to lure us into the expensive luxury of print.”

We have already stated that we value these lectures as being the record of the experience of a sound medical practitioner, who has had at his disposal great opportunities, of which he has fully availed himself. It strikes us that, at the present moment, when we fancy that we have witnessed rather a contempt for the principles there

enunciated, that we may, perhaps with advantage, reproduce the following expression of his experience upon a question of great importance in some of our surgical operations:—

“In some cases of amputation, the femoral vein bleeds so profusely that it is absolutely necessary to tie it. This profuse venous hemorrhage generally occurs in feeble subjects long wasted by disease. The vein dilates under the upward column, the valves become useless, and the regurgitation is fearful. But do not think, gentlemen, that it is a matter of light importance, whether the vein is tied or not. All the older surgeons of the present day held it to be a very dangerous practice; and I am convinced that that opinion was not lightly embraced, but was the result of observations on the serious mischief following this step. In modern times it has been the fashion to assert that there is no danger whatever in applying a ligature to a vein. My own opinion is, that both opinions are in the extreme. I can state, from the experience of more than thirty years in this large hospital, that veins may be tied with impunity; but I am also bound to add, that I have seen phlebitis follow the use of the ligature, and a fatal result the sequence, if not the consequence, of the practice.

“Indeed, I believe this very case is an illustration of the mischief which sometimes ensues from it.

“It is quite true that we found sufficient visceral disease to account for this poor fellow’s death, without being obliged to look at the state of the veins to explain it; and it is also true that phlebitis and pyemia occur in subjects when no vein has been ligatured. Still, it is my duty to call your attention to all the facts of the case, that they may be stored by you, and compared with the other facts you may be able to obtain on this subject during your sojourn in this Borough of Southwark; and I must again repeat, that a ligature should not be put on a vein unless the hemorrhage from that vein is so great as to endanger the safety of your patient.”

Mr. Solly’s observations upon stricture, whilst by no means exhaustive of the subject, are full of practical value; but, perhaps, none of them are of greater value than these contained in the following extract. Too often have we seen all but irreparable mischief follow the neglect of the principles here enunciated; and fain would we impress their importance upon the minds of all who venture to undertake the management of what frequently prove the most difficult cases to treat in the range of surgery:—

“There is another thing to be remembered in the treatment of stricture: never be ashamed to leave the bedside of the patient without succeeding

in passing a bougie. The late Mr. Copland, who was as sound and practical a surgeon as any in his day, said to a patient, who afterwards came under my care, with a very irritable stricture and false passage, 'Mind you never place yourself under the care of a surgeon who is afraid to acknowledge that he cannot always succeed in passing a catheter.' I am told that a hospital surgeon, now deceased, passed a sleepless night from vexation if he failed to introduce an instrument into the bladder in the presence of his pupils. Such a man must have made many a false passage. Of such a proceeding you should have the most intense horror. Every good surgeon will fail occasionally in the introduction of a bougie, but no good surgeon ought to make a false passage, though a skilful surgeon will sometimes do it, when his temper or his pride rules his hand instead of his reason and his conscience."

We have selected these passages for our readers' perusal, not that we by any means consider them the most important of Mr. Solly's experiences. The whole work is a satisfactory exposition of his views upon many of the most important topics in medicine and surgery; and we say it advisedly, one without which no medical man's library can be considered complete. Most cordially do we commend it to our readers' notice.

The Antecedents of Cancer. By CHAS. H. MOORE, F.R.C.S.
London: Richards. 1865. 8vo, pp. 53.

Primary Cancer of the Brain. By G. MACKENZIE BACON, M.D.
London: Churchill. 1865. 8vo, pp. 27.

On Intra-thoracic Cancer. By JOHN COCKLE, M.D. London:
Churchill. 1865. 8vo, pp. 159.

On Cancer, its Allies and Counterfeits. By THOS. WREEDEN
COOKE, Surgeon, &c. London: Longmans. 1865. 8vo, pp.
226.

THE greater part of the argument which constitutes the subject matter of Mr. Moore's pamphlet, we are informed in the preface, was presented to the British Medical Association at their last annual meeting, at Leamington; and was designedly made brief, and chiefly suggestive, being prepared at the request of the Committee of Council, for the purpose of introducing a discussion

on the subject—"Are there any antecedent conditions influencing the production of cancer?" With a view of precisely limiting the conditions and the period of time to which his enquiries should extend, the author has given the following definition of what he considers should be included in the term cancer:—"It is," he says, "a disease originating in a single spot as one tumour or new growth. In that spot it mixes with the natural tissues, which are destroyed in it, and never reproduced; its component microscopic cells having a greater power of multiplication than those which it displaces, but less of vital stability. The tumour is liable to early degeneration, ulceration, and gangrene; its elementary parts are capable of filtering or being shed into the adjoining healthy tissues, and they may thus be dispersed over a very wide area; they may also be conveyed to parts remote from their place of origin, travelling with the circulation, or sinking by gravitation in the liquids of a serous cavity. Afterwards, when attached in a new site, they manifest a power of growing, as they might have done where they first sprang. In each new place they thus give rise to a separate tumour similar to the first. The disease, so far as we know, is common to men of all nations, all times, and all parts of the world; and it is not limited to the human species, being met with also in the domestic animals, both herbivorous and carnivorous. These qualities and characteristics are not all possessed by every example of the disease; and, happily, the most virulent are the most rare. It is in some cases limited to the part in which it first arose; whilst in others, not apparently dissimilar, it becomes widely dispersed. Every degree of malignancy in this respect is, in fact, observable in different cases; from that which remains for twenty years a local, albeit an advancing disease, to that which is diffused all over the body in as many weeks. The solid growth which slowly creeps over the face, and is followed so closely by ulceration that the substantial disease of the margin rarely exceeds the thickness of an old penny-piece—that growth misnamed an ulcer, which scoops such hideous caverns in the human visage—is no less a cancer than the occult tumour which forms in the male or female mamma, and which, when 'open,' as we term it, has a like ulcer with only a thicker edge. The capacity for diffusion may be possessed by all cancers, though not that of growing when diffused. The rodent cancer, and some of the mammary, may yield up to the absorbents no more of their substance than a fatty tumour, the elements of which, when taken into the

circulation, display no power of reproduction in the next place they come to occupy. As cancer they may never pass out of the vicinity of the original tumour, or reach beyond the glands; yet, in the peculiarities of their local progress and in their common hostility to every indigenous texture, they are equally cancerous with those which can spread to distant regions and luxuriate in dissimilar organs, and which possess sufficient vitality to grow after transplantation, not only from one organ to another, but from one to another animal."

In the succeeding pages the author fully and fairly considers the two sides of the important question of antecedent influences upon the disease which he has thus broadly and comprehensively defined. He conceives that the grounds on which cancer is held to be originally of constitutional nature are these:—Its universal diffusion throughout the body; its occasional commencement in many primary tumours simultaneously; its capacity to grow in various textures; its local recurrence after an operation on the primary tumour; its appearance in internal organs, notwithstanding the extirpation of the primary tumour; its repetition in families (hereditary nature); and its relation to tubercle. The various arguments which have been, or may be adduced, in support of these several grounds of the opinion above stated, are carefully weighed and examined; and the author finally declares that he considers them as affording no sufficient reason for the opinion that the disease in question is originally constitutional. Nay, more; he adduces others which tend to establish the opposite opinion, and which he considers to be more consistent with facts. The reasons for opining that cancer arises as a local disease, independently of a constitutional or general cause, are thus stated:—1. Because of its invariable origin as a single tumour. 2. Because of the manifest dependence of the later tumours upon that first. This opinion is supported by the observed similarity of the morbid substance, in whatever organ or texture it may grow; by the order regulating its dissemination; by the interruption of the progress and dispersion of the disease if the primary tumour be removed; and by the possibility of extirpating that tumour by an early and adequately extensive operation. 3. Because of the remarkable manner in which it is inherited as a local and not as a constitutional peculiarity—a disease of the corresponding organ of plural members of one family—whilst at the same time any inheritance of the disease is uncommon, and that by infants, extremely rare. 4. Because of its preference of the healthiest persons.

The statistical and other evidence adduced in favour of the views entertained by the author are most important and instructive, and will be read with much interest. In recommending this essay as one well worthy of perusal we cannot resist transcribing the final paragraph:—

“There is one conclusion,” he says, “from the facts brought forward in this paper, which, though collateral only, and not contemplated at the institution of the enquiry, is yet of too great importance to pass unnoticed. I allude to the valuable argument furnished by them for early operations in cancer, and an argument which corresponds with their comparative success. Whilst, yet, the first tumour has undergone little diffusion, there is a hope of complete extirpation of the disease—a hope which quickly fades away as the tumour grows. I know nothing more distressing in practice than to meet a patient who has carefully concealed this disease till she could bear it no longer, with a refusal to operate on it at all; yet this is the too frequent result of delay on the part of patients in consulting a surgeon, and of early uncertainty as to the nature and probable progress of the disease. The conclusion at which I have arrived may, possibly, be of some advantage, if it only remove from the minds of patients the dread of discredit, which too many suppose to attach to having cancer; for, in fact, the power of generating the disease is rather a proof of a stronger than of a tainted constitution; and, on the whole, its appearance, in one member of a family, is likely to prove the exemption of all the rest. Any pain connected with the avowal may thus be mitigated, and the fullest advantage of treatment may be gained by an earlier recourse to medical aid.”

Dr. Mackenzie Bacon, assistant physician to the Cambridgeshire county Asylum for the Insane, having had his attention drawn to the subject of cerebral cancer by his incidentally meeting with a malignant tumour while examining the brain of a lunatic, instituted a series of enquiries into the *post mortem* records of the London hospitals, with a view to determining the frequency of occurrence of this disease. The pamphlet which is now before us furnishes us with the result of his investigations, and will be found a useful and valuable contribution to the pathology of the disease in question, treating as it does of the development of a morbid growth under circumstances and in a situation where no inconsiderable difficulties are opposed to its diagnosis, and where it is often impossible to decide on the nature of the affection till this shall have been revealed by *post mortem* examination.

In the course of his interesting preliminary remarks upon the subdivisions, definitions, nomenclature, and theories relating to cancer, the author illustrates the great value to be attached to the microscope as the most reliable agent in determining the true position of any growth in the catalogue of disease, by a reference to a case related by Dr. Hughes Bennett, who in describing a tumour attached to the tentorium and pressing on the cerebellum, represents it as composed of "an agglomeration of imperfectly formed bodies, usually denominated tubercle corpuscles. From this circumstance, as much as from the occurrence of old tubercular caverns in the lungs, we can have little hesitation in considering its nature to be tuberculous instead of cancerous, as it was first supposed to be by all who saw it. *Many such growths have, doubtless, been recorded as cancer of that organ.*" In collecting the materials for the statistical tables of the several hospitals, the author has made due allowance for the advances made in microscopic studies during later years as compared with the comparatively less valuable nature of observations made twenty years ago; and proposes accordingly to confine his researches to cases observed within ten years. "Taking," he says, "a period of ten years (1854-1863), I have myself searched the *post mortem* records of eight of the chief London hospitals, in order to discover the number revealed by dissection. The different methods pursued at the various hospitals prevent a comparison of the results on any uniform plan, and accordingly the statistics of each institution are given separately."

For further information on the subject the author has searched the museums of the London hospitals; but although most of them have been found to contain specimens of brain cancer no "distinction is made in their description as to the coexistence of similar disease elsewhere in the body." He next refers to published cases, of which, notwithstanding a diligent and continued search through the serial publications of many years past, the *Pathological Transactions*, and other works bearing on the subject, he has been able to collect but one-and-twenty—a further proof of the great rarity of the affection. The first six cases have been derived from hospital records; the remaining fifteen have been previously published. These twenty-one cases "suggest some points worth notice as regards the sex and age of the patients; for, of the whole number, no less than fourteen were male and only seven females, while four-fifths of the sufferers, from what Cruveilhier called the 'appanage

of advanced age,' were under forty. Of the twenty-one cases, five were under the age of twelve; two between twelve and twenty; four between twenty and thirty; five between thirty and forty; one between forty and fifty; three between fifty and sixty; and one had reached sixty-two."

The treatise on Intra-thoracic Cancer, by Dr. J. Cockle, is divided into two parts, the former of which is devoted altogether to the literature of the subject; this introductory part contains a most elaborate and interesting sketch of the labours of medical writers, beginning from the time of Doctor Thomas Willis, whose *Pharmaceutice Rationalis*, published in 1679, abounds in practical observations upon intra-thoracic diseases. This writer was the earliest observer of a particular sign of cancer of the lungs, which has since arrested very forcibly the attention of many who have carefully watched the disease, viz., a peculiar fetid condition of the breath. The subject is traced consecutively through the works of numerous succeeding authors, down to the fifth and concluding volume of Cruveilhier's book on Pathological Anatomy (1865), which treats exclusively of cancer and cancroïd formations. This historical sketch, which is well worthy of perusal, has been put forward, as we are reminded by the author, "simply with the view of tracing the various progressive steps of science in relation to points of clinical and pathological importance, and of detailing such individual cases as appeared to possess a special value in connexion with the subject." It does not profess to exhaust the subject of intra-thoracic cancer, even in its literary aspect; however, for the convenience of those who may desire to investigate the subject more thoroughly, we find appended to this part of the work a very complete bibliography, containing in fact a list of twenty-seven Latin, fifty-one French, thirty-seven German, three American, one Italian, three Dutch, and forty-three English works, in addition to the references given in the commentaries on the author's own cases in the course of the succeeding part of his treatise.

Part the second consists of contributions to the pathology of intra-thoracic cancer, and appears to be an amplification of former writings which have been already published by the author in several of the journals; in its present shape it is a very complete and valuable treatise on the subject. We would direct the careful attention of our readers particularly to that portion of the work which treats of the diagnosis of intra-thoracic cancer from such affections as chronic pleuritis, aneurism, subacute or chronic pericarditis

chronic or interstitial pneumonia and cirrhosis of the lung, hydatids or simple cysts, and hepatic cancer.

The last work on our list, namely, that of Mr. Thomas Weeden Cooke, opens with a very excellent practical observation, which is quoted from Dr. Hughes Bennett, of Edinburgh:—"If every young practitioner," says the learned professor, "would dedicate his life to the careful elucidation of the natural progress of only one disease, he would do more for medical practice than has been accomplished by centuries of empirical trials of remedies;" and, in accordance with the spirit of this advice, Mr. Cooke appears to have been occupied in improving to the utmost the opportunities which his position as surgeon to the Cancer Hospital has afforded him of studying the characteristic features of this disease, its clinical history and associations, and its treatment. The principal purport of the present work appears to be the establishment of a theory of the origin of cancer. The author looks upon this disease, if we mistake not, as depending upon a vice of original organization, and perfectly independent of any of the acquired habits or circumstances pertaining to our daily life; not that he excludes the operation of such agencies as are usually denominated exciting causes; on the contrary, he admits that many of these have the power of calling the disease into action, "but only at an earlier period than it would have shown itself had there been no local injury to determine and hasten the development." In short, its *seat* may be determined in many cases by exciting causes, but its *source* is hereditary predisposition, and that only. Moreover, the author holds very strong and decided views with regard to the relationship of cancer and tubercle. There is, he says, "in truth, a constitutional cancerous diathesis, as there is a constitutional tubercular diathesis; and as I shall have occasion to show, presently, there are strong grounds for believing that these two depraved conditions of the system have a more intimate relationship than is generally supposed." This relationship is more pointedly indicated further on, where he says:—"For a long time I had looked upon the occasional combination of tubercle and cancer as a curious coincidence, which did not seem to have any practical bearing upon the enquiry as to the nature of cancer. I had seen syphilis and cancer in the same person in rare instances, and the fact of phthisis being a frequent disease in our climate seemed to make it quite natural that occasionally our cancerous patients should also be phthisical. I subsequently noticed that adenoid tumours nearly always

occur in persons who had phthisical relations; and further, that adenoid tumours sometimes become cancerous. Stimulated by these associations, I have made diligent enquiry, and I find, in a very large number of instances, that phthisis exists in the families of most cancerous patients, who have not an hereditary tendency to cancer itself; and that a cancerous parent will often beget children that become phthisical. In another part of this treatise I shall give instances in support of this assertion. At present, in order that I may not break the chain of my argument, I must beg the reader to admit for a moment that the fact is as I have stated. If, then, phthisis is found to precede and to follow the evolution of cancer in a very marked degree, and if it be a constant *associate* of tumours which *occasionally* take on cancerous action, *how are we to escape the conclusion* that these two diseased products, cancer and tubercle, are *interchangeable creations*? Thus may we fill up the hiatus wanting in the statistics of the hereditary predisposition to cancer, and thus only may we safely and surely build up the theory of the constitutional origin of cancer."

We must be permitted to say that the conclusion above indicated does not appear to be a legitimate *sequitur*. The author has plainly confounded associated conditions with interchangeable existences, or he would not have considered it a matter of difficulty to "escape the conclusion" to which the above cited sophism appears to lead!

In several of the succeeding chapters, the author describes a variety of cancerous and other diseases which affect the breast, the tongue, the lips, and face, &c. In addition to a number of cases illustrative of diagnostic points attended with doubt or difficulty, the book is embellished with very fine lithographic plates, most carefully coloured with the hand; in fact, the general appearance of the work is quite in accordance with Messrs. Longman's usual good taste.

PART III.

MEDICAL MISCELLANY.

Reports, Retrospects, and Scientific Intelligence.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.*

DR. M'DOWEL, President.

Intra-thoracic Aneurism.—Mr. E. HAMILTON exhibited a specimen of intra-thoracic aneurism, taken from the body of a member of the constabulary force who had been in Steveens' Hospital since last November. When admitted he was suffering from intense and urgent dyspnea. He was obliged to sit propped up in bed, and was unable to lie down at night without great suffering being induced. He appeared pale and emaciated, as if he had gone through a protracted course of illness. He had severe palpitations of the heart, and vertigo. On examining him it was found impossible to detect the least pulsation in the wrist at the right side, or even in the arteries of the limb higher up. He suffered from cough, which occurred in paroxysms, and his breathing was stridulous.

The history which he gave of himself was that he had been for a long time ill—in fact, ever since he entered the Constabulary force, and he attributed his ailments to the severe duty he had to perform. He had been several times in the provincial hospitals, and was treated for attacks of pain resembling those of pleurisy.

A careful examination was made of his chest. There was some slight dulness, on percussion, towards the upper part of the right lung. There was no external tumour or any enlargement on which could be founded an accurate diagnosis, but there was a slight elevation of the cartilage of the second rib. It was impossible to detect any bruit, and yet, on pressing the finger above the sternum it came in contact with a tumour, which communicated to it a distinct sense of pulsation. The opinion

* These reports are furnished by Dr. R. W. Smith, Secretary to the Society.

was hazarded that the man suffered from aneurism of the arteria innominata, or that portion of the arch from which it springs. He also suffered from pain in the back, and epigastric palpitation; and in the latter region they could detect, by pressure, a distinct *bruit de souffle*. The patient continued in hospital, but nothing could be done except to palliate his sufferings. About three weeks ago he complained of a severe burning pain in the neck below the larynx. This was relieved by the application of sedatives. In about a week before his death he had an attack of hemoptysis. Under the use of acetate of lead it subsided, but returned at intervals, until last Saturday, when, sitting up in bed, he suddenly threw back his head, blood flowed from his mouth, and he expired.

The *post mortem* examination showed a considerable enlargement of the arch of the aorta, from the upper or right portion of which, where the innominata arises, there sprung a pouch which extended over to the origin of the left carotid. This aneurismal tumour pressed on the trachea, and had leaked into it; they found erosion of the cartilages of the trachea in its anterior aspect, and a large clot of blood extending into the bronchial tubes.

They found here an explanation of the absence of the pulse in the wrist. The subclavian artery on the right side was completely closed; there was no passage of blood from the innominata into it; the opening into the carotid was pervious. The heart and its valves were sound. Circumstances did not admit of the examination of the abdominal aorta, but the thoracic aorta was distinctly enlarged just at the place where it passed into the diaphragm.—*February 3, 1866.*

Amyloid Degeneration.—DR. PURSER said:—The specimens which I would bring under the notice of the society consist of the liver, kidneys, and spleen, taken from a body in the dissecting room of the Carmichael School, and presenting, in a remarkable degree, the pathological condition known as amyloid or lardaceous degeneration.

The subject from which they were taken was a man of middle age, dark complexion, and powerful frame. His muscles were remarkably well developed, and the subcutaneous fat was abundant—there was no dropsy. Over the left side of the chest were the marks of recent cupping, and on his forearms various nautical devices were tattooed. He had all the appearance of a man who had died of acute disease: the base of the left lung was consolidated, and the left pleura presented the signs of recent inflammation; in the apex of each lung were the remains of old tubercles, which had undergone the calcareous degeneration. The heart was greatly enlarged, but its valves and muscular tissue were healthy; the liver was very greatly increased in size—the enlargement involving chiefly those parts which lie to the left of the fissure for the gall bladder—there were no morbid adhesions binding it to neighbouring

parts; its surface was in part smooth, and in part marked by deep cicatrices, which all tended to assume a stellate form; its edges were rounded: the gall bladder contained about two drachms of thin, yellow bile. On section, the liver appeared very evidently composed of two substances, one opaque, and of a yellowish-white colour, the other pink and translucent. In some places there were masses of cartilaginous consistence, and of a whitish, semi-transparent, nearly homogeneous appearance; these occupied, for the most part, the inferior surface and thick edge of the liver, and lay invariably near the surface; their limits were pretty well defined; they varied in size from about half an inch to two inches in diameter. The liver was remarkably bloodless, and by pressure or scraping but little juice could be obtained. When thin sections of the organ were examined under the microscope, the opaque portion was seen to consist of cells, rendered dark by the accumulation in them of granules which concealed thin nuclei; by ether much of this was shown to be fat; but even after the action of this reagent the cells remained more opaque and granular than healthy liver cells; the translucent portion was composed of vessels of a peculiar shining appearance, and of cells which had lost their nuclei, and by fusion one with another had become converted into shining flakes, resembling pieces of rough ice. On applying a solution of iodine, the white portion was unaltered, while the translucent portion became deeply stained of a reddish-brown colour, and the parts of which it was composed were clearly seen; the vessels were everywhere thickened, their calibre diminished, and they remained open when cut transversely. The relations of the amyloid and fatty portions of the lobules seemed to vary; in some cases the former occupied the periphery of the lobules, following the distribution of the portal vein, in others it surrounded the intralobular hepatic vein, and in a few instances it occupied the zone of distribution of the hepatic artery; in these cases the lobule consisted of an outer fatty zone, a middle amyloid zone, and a fatty centre. This arrangement was quite exceptional, although Virchow states that it is most frequent. The cartilaginous portions become everywhere of a deep brown colour with iodine; here the vessels seemed to be impervious, and the cells universally degenerated. As well as I could judge, in the liver the degeneration appears to have commenced in the vessels, and spread from them to the glandular structure. There was but little fibrous tissue about the cicatrices, and this gave no reaction with iodine. It is probable that the scars were caused more by atrophy and falling in of liver structure than by effusion and contraction of lymph.

The spleen was somewhat enlarged, but of normal consistence. Under the microscope the vessels were seen thickened and transparent; they stained deeply with iodine, as did also spots of the parenchyma.

Scattered through the spleen were some white bodies, about the size of the head of a small pin; these were composed of cells and nuclei; did not stain with iodine, and were probably enlarged Malpighian bodies; besides these, I failed to discover any Malpighian corpuscles.

The kidneys were large and heavy; the capsule stripped off readily; the surface of the gland was rough and granular, and presented in places a stellate vascularity; on section, the distinction between the cortical and medullary portions was badly marked, and the whole gland substance was much increased in depth. In a good light the Malpighian tufts could be seen as minute shining points in the cortex. On applying iodine to a thin section, the whole arterial system of the kidney became of a dark, reddish-brown colour; the arteries were much diminished in calibre, their walls irregularly thickened, so as to produce an appearance which has been aptly compared to that of the ipecacuan root. Not only were the arteries connected with the Malpighian tufts degenerated, but also the vasa recta. The tubes were in different conditions; some irregularly distended by opaque fatty granules, some filled by a homogeneous transparent material, and some collapsed and empty, while a very few were tolerably healthy; the tubes of the pyramids were for the most part normal. In the kidney the amyloid degeneration seemed not to have extended beyond the vessels; certain it is, that neither the fatty cells nor the translucent masses expressed from the tubes gave any reaction with iodine. There were on each side two renal arteries, one arising in the usual situation, the other smaller, coming off from the aorta about an inch higher up, and giving off the inferior capsular; on both sides the arteries entered the pelvis anterior to the veins, which were single.

The supra-renal capsules were enlarged slightly; the cortex was yellowish-white; the medulla firm, translucent, and of a reddish-grey colour. On the application of iodine numerous red streaks were seen running vertically through the cortex, and also some in the medulla; these all appeared to be blood vessels. The other parts were not examined.

Notwithstanding the absence of any clinical history of this case, it presents some points of interest. How a man whose organs were so much diseased could keep up his *embonpoint* and muscularity, is not a little remarkable, but still more remarkable is the absence of dropsy; for here we had not only such an amount of capillary obstruction as to produce a very considerable hypertrophy of the heart, but also disease of the blood-forming and purifying glands sufficient to induce that condition of blood most prone to give rise to dropsical effusion. This case, furthermore, shows on what a slender thread hangs the life of a person whose excretory organs are diseased; for the pleuro-pneumonia, of which we may fairly suppose he died, was quite insufficient to kill one whose organs generally were healthy.

Amyloid degeneration is almost invariably associated with either syphilis, tuberculosis, or disease of the bones. It is generally believed to be a constitutional affection ; but Wilkes records a case where it followed disease of the femur produced by injury in a previously healthy man ; and Billroth mentions an instance where it was confined to the lymphatic glands nearest to a diseased humerus. In the present instance the bones were unaffected ; there was no evidence, beyond the degenerated viscera, of syphilis, and the tubercles in the lungs had evidently been inactive for years. With regard to the nature of the substance found in the degenerated parts considerable uncertainty still prevails, owing to the difficulty of isolating it perfectly pure. It has been analysed by Friedreich, Schmidt, and others, who always found it to contain nitrogen in considerable quantity ; and Dr. R. Macdonnell, who has isolated it in some quantity, failed to convert it into sugar. It hence differs from the physiological amyloid, such as is found in the healthy liver, and in the tissues of the fetus, which, although it gives also a red reaction with iodine, agrees both in chemical constitution and in all its properties with vegetable dextrine. The substance which seems most allied to the pathological amyloid is chitine, which is obtained from the coverings of crustacea, arachnida, and insecta ; this, which contains nitrogen, has, after many failures, been converted into sugar by M. Berthelot ; this chemist looks on it as a compound body, consisting of a nitrogenous substance allied to albumen, and hydrocarbonaceous substance related to the starch group.—*February 10, 1866.*

Ossification of the Dura Mater.—DR. MARCUS EUSTACE exhibited a specimen of osseous deposit in the falx cerebri, taken from the body of a gentleman who died in July last, having been two years and three months under the care of the Drs. Eustace, in their private asylum, Hampstead House, Glasnevin.

The case at first was very obscure. About three years before his death he was under the impression that he had disease of the brain, and that he would die suddenly ; and the late Dr. Mayne and Dr. Banks, having been consulted, recommended that he should travel. He went to Edinburgh and London, and sought additional advice there, as he suffered at night from occasional faintness ; and at both places he received the opinion that there was nothing serious the matter. He soon after showed that he had the fixed idea that he had died on a certain day, and resisted taking food, acting in every way he could consistently with his delusion.

He was brought from London to Dublin ; and on further consultation with Drs. Banks, Mayne, and Gordon, it was thought there was some obscure disease of the brain.

There was a slight tendency to paralysis, at this time, on the left side,

which soon partially passed off. He did not take sufficient food, and wished to remain in bed. He was brought to Hampstead House on the 21st of April, 1863; he was then a good deal reduced, and required assistance in walking; had a rapid pulse, and was feverish. Acting in accordance with his delusion that he was dead, he would not acknowledge that he suffered any pain. During the first ten days he had food barely sufficient to support life, and then refused to take any, and it was found necessary to introduce it by the stomach pump. The greatest difficulty was experienced in opening his mouth. The nasal tube, as recommended by Dr. Harrington Tuke, was tried, and on each occasion it appeared to pass into the larynx.

Dr. Mayne was consulted, who tried over and over again to pass the tube, but with the same result, and was perfectly convinced that it was useless to make any further effort to introduce it. It was found that the only means of success was by tiring the masseter muscles by attempts with the handle of a spoon, which caused him to clench his teeth, so that ultimately the muscles became fatigued, and relaxed sufficiently to enable them to open his mouth to pass in the plug.

The stomach pump was used three times a day; the catheter also required to be used, and cold water enemata were given to produce action of the bowels, all of which he resisted with great force.

After four months, during which time this mode of treatment was constantly pursued, from being greatly reduced the patient had increased considerably in flesh; he had riding exercise, as he could not walk, being accompanied by an attendant. He continued to increase in flesh; and one day, seeing one of the servants eating, he said he could eat also; he was in the habit of saying it was a shame to bring a dead man about, and that he ought to be buried, so that his family might get his property, his life being largely insured. He now yielded, and took food freely, but said it was a strange thing for a dead man to eat. He gradually recovered; and after nine months' residence at Hampstead House, he appeared to be perfectly well. He still had the same kind of gait, but walked without difficulty two or three miles daily. His mind appeared to be quite restored; his delusion passed away; and on his friends visiting him, he frequently said, "I am still alive, and my life has been saved by the use of the stomach pump."

One day, while sitting in the drawing-room, he was remarked by the servant to sink, sliding off the chair; he was at once removed to bed, and within a few minutes had a severe epileptic fit, which was repeated throughout that day and night, each half hour. There was great prostration, the pulse 120, the breathing quick, sordes on the teeth, and insensibility. The following day he was something better, but continued to have convulsions every half hour. When they would pass off he would appear in his right mind. While conversing, the convulsions

would come on, draw him up for a moment, and relax again, and he would then continue the conversation precisely where he had left off, apparently unaware of the attack. This continued four days; during that time his head was leeches and blistered, mercury was administered, sedatives were also given, and the catheter was used. On the fourth day a large belladonna plaster was applied along the spine. From that time until three months after he had no convulsion; and whether that was the result of the belladonna plaster, or that the convulsions were on the point of subsiding, cannot be decided. About a fortnight afterwards the delusion that he was dead again took possession of his mind; and after three months he had another attack of epileptic convulsions, which lasted for three days, and passed off like the last; but in this instance belladonna was applied without producing any good effect. Again he recovered, but with partial paralysis of the lower limbs, and he was seldom without twitchings of the muscles of the neck and face; and about eighteen months after he came to the asylum it appeared that he was undergoing a great change for the worse. The spasms were more evident and increased; he took barely sufficient food; he slept but little, requiring draughts of chlorodyne frequently repeated; and in July, 1865, there was a fourth attack. After death the ossified falx cerebri now exhibited was discovered. The brain was perfectly healthy, and was moulded exactly to the nodules of the bony deposit. The spinal marrow and all the other organs were perfectly healthy; there was but little effusion or congestion. The case had been treated as one of reflex epilepsy, and bromide of potash was given all through with marked benefit.—*February 10, 1866.*

Vesical Calculus.—Dr. FLEMING said, that the stone he now presented to the society had escaped from the bladder a few hours after the operation of "Lithectomy" had been performed for its removal. The subject of it was a girl, aged eleven years, who had lately applied at the Netterville Institution, complaining of some uneasiness and difficulty in micturition, occasionally amounting to almost complete retention of urine, and often accompanied by a sudden interruption to its current. He was at first not disposed to attach much importance to the statements the child had made, imagining that they might be exaggerated, or were attributable to some intestinal cause, such as ascarides in the rectum, or other irritation in that locality. He ascertained that there was no congenital defect present to account for them. The urine was carefully examined—its reaction and density were natural. It deposited much lithate of ammonia, and numerous crystals of lithic acid, and many of oxalate of lime, especially attracted attention in the microscopical examination. The child was now attacked with painful retention of urine, which required the use of the catheter; and in consequence of its recurrence the child was admitted into the Richmond Hospital, where,

on not a few occasions, the sensation of a calculus striking against a gum elastic or metallic instrument was distinctly felt by Dr. Fleming, and several of the students, especially if the child coughed or cried during the escape of the urine. The capacity of the bladder was remarkably large, often containing a pint and a-half of urine, and often very tolerant of the pressure of such an amount, from the fear the child had of the use of the catheter, in consequence of some difficulty in its introduction, arising from the concealed situation of the orifice of the urethra.

The rational signs of stone were, in a great measure, persistent; the physical were not by any means satisfactory. It was decided to dilate the urethra to an extent to admit of a careful exploration of the bladder, and a piece of tangle bougie was prepared for the purpose. It was made conical in shape; answered on the scale to about No. 4 or 5; was four inches or so in length; and was curved at its proximal end to prevent its escape into the bladder, and admit of its being properly secured in situ. By previous experiment, this piece of tangle bougie was found to be capable of expanding so as to reach No. 17 on the scale. On the night previous to operation it was passed into the bladder, and properly secured. Under the influence of opiates, the bougie remained tolerably stationary until the following morning, when, in the removal of the child to the operation theatre, it escaped, but yet had effected dilatation to an extent which permitted the passage of Weiss's female urethral dilator with the greatest ease. With the assistance of his colleagues, Drs. Hamilton and Smith, and under chloroform, the dilatations were increased by the ordinary screw movement; and on the removal of the dilators Dr. F. was able to pass his fore-finger freely into the bladder, having had recourse to this expedient at the moment, in consequence of the rapid escape of the urine, which would have been increased if a forceps or scoop had been used. Dr. F. could feel the inner surface of the bladder with care, and it communicated the sensation as if the body of the organ was thrown into longitudinal folds, and the inferior fundus so much deepened that the point of the finger did not satisfactorily reach the mucous surface. No stone could be felt with the finger, nor could the presence of one be detected with a forceps subsequently introduced. The child was removed to bed, and for a short time after the operation suffered from the usual effects of chloroform, such as, some slight delirium and sickness; but these quickly subsided under the uses of mild stimulants.

Urine escaped throughout the day, but some uneasiness was experienced from frequent paroxysms of tenesmus; what escaped resembled small masses of coagulated blood mixed with urine. During his visit, about six hours after the operation, one of these paroxysms occurred when, on separating the labia, he found, in a coagulum which had just passed, the calculus now exhibited; it was nearly spherical in shape, and too large

to pass through No. 17 on the catheter scale; it was rough on the surface, and, when examined with a magnifying glass, its several points, having a crystalline aspect, were seen. According to Dr. Grimshaw, it consisted of oxalates of lime, mixed with a considerable quantity of organic matter. Now the urine escaped, as the little child described it, in large gushes, not easily controlled when the desire to urinate supervened, and with some, but severe, pain. In the intervals no urine escaped involuntarily; during sleep there was some incontinence of urine, and the same when she coughed, or made any such exertion. The sufferings of the child after the operation were very trifling; there was no fever of any moment, neither was there any appreciable amount of local suffering. Within ten days, at her own desire, she was removed home, and improved from day to day; and was able to move about, without annoyance, three weeks from the day of the operation. The bladder is gradually recovering its normal action; incontinence of urine rarely occurs, and then only during sleep; and all former painful sensations have ceased.

The practical interest and importance of this case require no comment. Here the operation of lithectomy, by simply dilating the urethra without any incision, answered all purposes; at the same time it was to be recollected that, from the hemorrhage which occurred, some lacerations must have taken place. Here the question of the presence of any other foreign body but stone was not to be entertained on account of the age of the child. Not the least interesting feature connected with this case, was the nature of the stone, oxalate of lime calculus being comparatively rare in the female.

Dr. F. added, that the great value of the tangle bougie, as a means of dilating the urethra in cases such as the above, can not be exaggerated. It can be easily formed into any shape that may be required, and the amount of the dilatation of which it is capable can be accurately measured previous to its use.—*February 10, 1866.*

Bright's Disease of the Kidney.—Dr. LYONS said the specimens which he now brought before the society presented features of unusual interest, but he could give only an imperfect history of the case. The patient, aged twenty-eight, was admitted on that day week into Jervis-street Hospital with symptoms of a very obscure character. He had a certain amount of unsteadiness in gait and want of confidence in his general movements, with occasional twitchings of the muscles in the extremities. He stated that he had been subject to vomiting for some weeks previously. He (Dr. Lyons) saw the man on the next day, and then noticed something very peculiar about him. He lay in bed, making little or no complaint; vomited two or three times, but yet seemed to have a desire for food; and having been helped to the ordinary dinner of the hospital, he seemed

to partake of it with appetite; but the food was hardly swallowed when it was returned. On examining the abdomen the bladder was found largely distended with water; and on the catheter being passed, 56 ounces of urine was withdrawn. It was noticed that the patient stood at the bed side with a certain unsteadiness of limb, which was partly attributed to the fact that the catheter was then passed for the first time. He had a confused idea in his mind that in consequence of vomiting so much fluid he had no necessity for passing water, and he experienced no inconvenience in consequence of the large quantity of urine that had accumulated in the bladder, nor was he aware of its presence. As long as the patient lived the water had to be drawn off by the catheter; and when examined it was found loaded with albumen, the specific gravity being 1014. The man was rather spare, and there was no dropsical effusion. He did not complain of pain; and the only statement he made of suffering was that of the vomiting, which had existed for some time before, and continued for a short time after his admission into hospital, but for the last two or three days of life there was no vomiting. On a day or two subsequently the patient began to exhibit symptoms of a still more remarkable character. It was now found that there was considerable increase of sensibility in several parts of the body. On attempting to move one of the lower limbs he complained of excessive pain; the soles of the feet were also painful when rubbed; he was able to sit up in bed, and even get out of bed, and made no complaint of suffering of any kind; he had no headache or other symptom that would call attention to the head. His pulse, on admission, was 96, and afterwards sank to 76; it was a full, slow, long, labouring pulse. There was no affection of the lungs, no irregularity in the action of the heart, nor anything observable in the cardiac sounds of an abnormal kind. On the second day before death he lay with his head drawn back, but there was no active muscular retraction or muscular rigidity; the tongue was now dry, brown, and furred, and the pulse 76. Mercurial ointment was now directed to be rubbed in, for at this period a strong impression arose that a certain amount of cerebro-spinal arachnitis was what would most fully explain the phenomena found in the case. The rubbing in of the ointment had to be desisted from on account of the pain which was experienced in the axillæ and the groins. The patient gradually sank, without, so far as he remembered, the development of any further symptoms of a remarkable or unusual kind. Thirty-six ounces of water were drawn off from the bladder on the day before he died, and found to be loaded with albumen. There was not the slightest œdema anywhere.

Dr. L. carefully examined the spinal cord, having made a section with the rachitome; and on opening the cranium and removing the cord and brain together, there was found an extraordinary amount of congestion in all the venous sinuses. A large amount of blood escaped in the operation

of opening the spinal canal, at the lower part of which a considerable amount of serous effusion existed, as well as in the sac of the cerebral arachnoid membrane.

When the abdomen was opened the kidneys were found to be very small, and advanced in Bright's disease. They each weighed not more than two and a-half ounces. The cortical portion was in that state of fatty degeneration which constituted the main element of Bright's disease of the kidneys, but the affection was not developed to the extent which was generally found in connexion with such symptoms as those exhibited in this case. The section of the kidney was examined under the microscope, and found to present a certain amount of fatty deposit in the tubules, but altogether a less considerable amount of fatty degeneration than was commonly seen in cases which had progressed as far as dropsical effusion and uremic poisoning. The spleen and liver did not present any unusual appearance.

On opening the pericardium two or three ounces of turbid serum, mixed with flaky lymph, were found, and here and there on the heart there was a slight deposit of lymph exudation, but nothing like general pericarditis existed. The heart was considerably hypertrophied, and the walls very much thickened, and this without any valvular lesion to account for it. The mitral valves, though rather short in their vertical direction, were still adequate to close the mitral orifice.

In no less than 23 out of 100 cases which had been recorded by Dr. Bright, there was a considerable amount of hypertrophy without valvular lesion to account for it; in 52 of his cases there was hypertrophy of the heart; but a certain number had disease of the mitral valves, and a certain number disease of the aortic valves—23 having hypertrophy without any disease of the valves.

In the lungs there was a well marked example of puckered cicatrix, where tubercle had existed and been healed. The right lung was adherent throughout, but not otherwise affected. On carefully examining the brain and spinal cord for anything that would account for the remarkable hyperesthesia, there was only found a certain amount of congestion on the surface of the brain, and of lymph opacity, but no free effusion of lymph in any part of the cranium or arachnoid. There was active hyperemia of the cord, but there was no lymph exudation, neither was there any marked alteration in the substance of the cord, or of the brain. To sum up, there was a certain amount of arachnitis of the cerebral arachnoid, in an early stage, with hyperemia, and a slight amount of lymph exudation. The spinal arachnoid was in a state of active hyperemia, with a very great venous congestion, and serous effusion. The escape of blood was so great that it was impossible to separate any portion of the serum for further examination.

As before observed, cases of renal disease are often seen going on to a

more extreme degree, so far as symptoms of dropsy and uremic poisoning are concerned, but with the production of a greater amount of renal lesion than in this particular instance.

The state of hyperesthesia was probably due to the hyperemic condition of the arachnoid in the brain and cord. It may be remarked that the patient did not present any symptoms of a tendency to coma or poisoning, except irritability of the stomach. There was no tenderness of the head or back; and the man, as he lay in bed, expressed himself as suffering, except during the last two days, and then only when he touched his thighs or the soles of his feet. It was an example of a renal lesion producing unusual symptoms, and without any dropsy, unless that in the arachnoid of the brain and cord.

Dr. L. observed that he met, recently, another remarkable case of unusual symptoms occurring in Bright's disease, but the dropsy to a large extent, and extreme headache. He pointed out a remarkable feature—the impossibility of selecting the proper words to express his ideas. Being asked one morning if he had slept the night, he replied that he had “slept about sixpence;” and on another occasion, when asked had he taken breakfast, he said he had “fasted on cobblers.” This dysphenic condition he had never met in any other instance of Bright's disease. He thought this case was well worth being added to the list of unusual cases which it was known to assume in various forms in Bright's disease. The absence of the peculiar gait, the muscular twitchings, and the hyperesthesia were exhibited in this instance, as well as the total absence of headache, and of the more ordinary symptoms of uremic poisoning, rendered it a case of a very unusual character.—*February 17*

Separation of the Symphysis Pubis, with Rupture of the Bladder.
 SYMES said, that on Monday, the 12th of February, a man, five years of age, fell from the roof of a new chapel, which was being built at Monkstown, a height of forty or fifty feet, and in falling struck across some timber—scaffolding poles—which were projecting from the walls. He did not see the man until eight days after the accident. The man was not brought into hospital until the 20th. His condition was most lamentable. He was suffering from great thirst and passed water, and on examining him he found a large tumour over the hypogastrium, and reaching up towards the umbilicus. It was like a distended bladder, differing, however, from it in that it was not symmetrical, there being more of it on the right side than on the left. On examining the tumour, he could trace it into the pelvis on the right side, and on the right as far forwards as Poupart's ligament. There was extensive ecchymosis, extending from the right iliac spine to the pubis; there was also an ecchymosis in the perineum, but he

no uneasiness in this situation, and made no complaint when pressure was made there. He kept his thigh semiflexed on the abdomen, and looked with horror on any movement of the limb, so much so, that he was induced to examine him for dislocation of the hip-joint, but found that there was no such lesion ; some of his ribs were injured. Just before he saw him the resident pupil had passed a catheter, and drawn off three or four ounces of urine. He (Mr. Symes) then passed the instrument, and succeeded, after a great deal of trouble, in drawing off about two ounces of very fetid urine mixed with blood and pus. The operation occupied about three-quarters of an hour, as he had frequently to clear the eye of the catheter from the clots which filled it, and obstructed the passage of the fluid. The urine came out dribbling, at uncertain times, and generally when least expected. This was followed by no sign of diminution of the tumour. The patient got a large allowance of wine, the usual opiates were administered, and he experienced some slight relief. He would not allow the catheter to remain in the bladder, so that it had to be introduced every four hours. He died on Saturday, the 24th, exactly twelve days after the injury. On the day before his death he could trace a distinctly fluctuating tumour over the right side of the pubis.

At the *post mortem* examination, on cutting through the abdominal muscles above the pubes, there was found a mass of putrescence. On getting into the abdominal cavity a great quantity of fetid urine and blood was met with, enclosed, as it were, in a sort of cyst formed of exuded lymph. This fluid had dissected its way between the peritoneum and the bones. It did not descend into the recto-vesical *cul de sac*. The amount of fluid was considerable, being over a quart. The intestines were matted together in the immediate neighbourhood, and a portion lay in front of the collection of matter, but the small extent of local peritonitis which was caused under these circumstances was remarkable. On examining the bones of the pelvis there was found a wide separation, even to the extent of three-quarters of an inch. At the symphysis pubis the fibro-cartilage had all gone with the left side, the bone being quite bare on the right side. The right os pubis was about half an inch below the level of the left ; the sub-pubic ligament and attachment of the triangular fascia had been torn through ; this evidently accounted for the great ease with which the catheter could be passed into the bladder. On examining the right sacro-iliac synchondrosis, a patch of effused blood was found beneath the muscles in the neighbourhood ; the joint was very movable, a great portion of the anterior ligament having been ruptured. On next directing attention to the bladder, two small rents were found in front, just above the level of the pubis, where the bladder is not covered by peritoneum. Mr. Symes said that a good deal of attention had been lately drawn to the subject of separation of the symphysis pubis with rupture of the bladder. Mr. Fleming, Mr. Hutchinson, of London, and Mr. Hamilton,

in his work on fractures, had collected several instances. This case was remarkable for the small amount of local peritonitis, and for the length of time he lived (twelve days) after receipt of the injury.—*February 17, 1866.*

Polypus of the Rectum.—DR. FLEMING said that he had, on other occasions, brought under the notice of the society cases of diseases of the anus and rectum, both of a non-malignant and of malignant character, and now wished to direct attention to a disease of the rectum of rare occurrence. He referred to polypus of the rectum. It was more frequently met with in children than in persons advanced in life, and hence it required careful investigation on the part of the surgeon. The subject of the present case was a little boy, aged about twelve years, who had been under treatment in the Richmond Hospital for malignant onychia of the great toe. Dr. Fleming was struck by the unfavourable results of the treatment adopted for its cure, and by the remarkably anemic aspect of the boy; when, subsequently, the child accidentally stated that he had piles, and that he had often bleeding during and after defecation. Now, hemorrhoids in children, whether external or internal, were of very rare occurrence. There were, doubtless, diseases in children which were improperly called piles, but these diseases could be distinctly referred to other specific causes, unnecessary here to particularize; and their diagnosis was easy. On examining the anus and rectum in the present case, no disease could be detected by ocular or by digital examination: when, during a sudden attack of prolapsus of the rectum, the appearance of blood on its mucous surface attracted attention; and, on removing the blood, two pedunculated tumours presented themselves, each growing from the front wall of the bowel by a narrow pedicle. These little tumours descended and returned with the prolapsus; they were about the size of a large garden pea, were spherical in shape, highly vascular, and had a solid and consistent feel. The treatment adopted for their removal was as simple as it was effectual; the pedicle of each was snipped across beyond a ligature, which was tied around its root; the surface was then washed over with a solution of the perchloride of iron, rest in the horizontal posture was enjoined, even during the action of the bowels; when, after a week, the normal functions of the parts were fully restored, and the general health was vastly improved. All were familiar with the special localities from which these polypoid growths sprang—such as the ear, the nose, the pharynx, and occasionally from the larynx; nay, more, the orifice of the urethra, both in the male and female, and the lining membranes of the bladder, occasionally gave origin to them. Dr. Fleming took the opportunity of showing an admirable drawing of this form of polypus growing from the interior of the bladder. The subject of the case was an aged

man, who, after frequently-recurring attacks of hematuria, sank anemic and dropsical. This man had been for a time a patient in Steevens' Hospital, and subsequently in the South Union Hospital, under the late Dr. Mayne; he laboured under the symptoms of the disease, especially violent attacks of hematuria, for many years. The structure of the polypus was fibro-cellular, and obviously it was non-malignant; its shape, situation, and outline have been accurately represented in the accompanying plate. Dr. Fleming alluded to some of those outgrowths

from the prostate gland, which often assumed a pedunculated form, and projected almost as isolated tumours into the bladder; excellent drawings of such were shown, as connected with the intestinal tract. He alluded to a remarkable case which was brought under the notice of the society by Professor Smith, and which was recorded in its *Proceedings*, where a thick pedunculated fleshy growth from the ileum passed into the colon and caused fatal obstruction of the bowel.

In the case of polypus of the rectum now before the society there was no irritability of the bladder; but in another instance, recorded by Dr. Fleming in the *Dublin Hospital Gazette*, the bladder was so irritable as to lead to the suspicion of the existence of a calculus, when, during the operation of sounding, prolapsus of the rectum took place and a large polypoid growth was detected, on the removal of which all uneasiness subsided.—*February 24, 1866.*

Hemiplegia, with Loss of Speech.—DR. BANKS said that on the 19th of February a man, aged twenty-four, was admitted into the Whitworth Hospital. As far as could be ascertained from the proprietor of the hotel in which he had been a porter, he was of regular habits, and not intemperate; he had not been known to be ill before the morning of the 19th. On the preceding evening he had been out, but returned

perfectly sober, and went to bed at his usual time. The following morning he did not appear, and a person going into his room to call him found him sitting up in bed, complaining much of pain in his head. Nevertheless, he got up, and about an hour afterwards he was found in a pantry where he had been at work, lying on his face, and perfectly insensible. On being roused and questioned, he put his hand to his head and attempted to speak; but the person who mentioned these facts to the resident pupil said he mumbled, and it was impossible to make out what he said; he was then brought to the hospital. On being roused and spoken sharply to, he attempted to reply, but he only uttered some disconnected monosyllables; he could not completely protrude his tongue, but he swallowed well, and he passed water voluntarily, even after coming into hospital. He was paralysed on the right side, but it was not complete; the right arm and right leg were paralysed, but he had some power of movement in the right arm. The pupils were equal, and he seemed, on being spoken to, to be intelligent; he made a movement as if he desired to do what was asked. There was a slight paralysis of the face on the right side—that slight and limited paralysis which is sometimes associated with hemiplegia. The heart was examined very carefully; its action was slow—about 58 in a minute; afterwards it fell to 52, but there was no abnormal sound; the urine was examined, but no albumen was found in it; sp. gr. 1016. The day following his admission it was found necessary to draw off the urine with a catheter.

His condition continued unaltered until the 22nd, when he suddenly became comatose, and very shortly afterwards died. The day before his death, Dr. Purser made an ophthalmoscopic examination of his eyes, the pupils being widely dilated by atropine. He found the fundus of each eye perfectly normal, with the exception of slight redness of the apparent outer portion of the left optic disc, which was, however, well margined. At the time of the ophthalmoscopic examination, Dr. Purser found him quite sensible when roused; but, when he attempted to answer questions, he only gave utterance to a few unintelligible monosyllables.

The *post mortem* examination in this case was performed by Dr. Purser. Some spots of extravasation were found under the tendon of the occipito frontalis on the right side; on the upper and right side of the occipital bone a small, irregular exostosis existed; the inner surface of the bones of the skull was elsewhere normal; the dura mater was healthy; the sinuses were gorged with blood, which flowed freely from the veins opened by incisions made parallel to the falx cerebri. No blood was extravasated on the surface of the brain. A few old adhesions of the arachnoid existed along the margins of the longitudinal fissure; there was also some thickening of this membrane where it covers the middle subarachnoid space, and passes from the anterior to the middle lobes across the fissure of Sylvius; there was no sign of recent inflammation;

the pia mater was not remarkably vascular. The middle and outer part of the cerebrum on the left side felt softer than the corresponding parts of the right side, which appeared to be healthy in every respect. The convolutions most affected were the posterior part of the second and third frontals, the inferior portion of the transverse frontal convolution, the island of Reil, and the anterior convolutions of the parietal lobe, and the inferior marginal convolution. The softening was not sharply defined, but seemed to pass from the outer extremity of the fissure of Sylvius, gradually merging into healthy tissue. On slicing off the hemispheres on a level with the corpus callosum, the grey matter of the softened convolutions was seen to be thicker than that on the opposite side; in parts it was quite diffuent. The white substance was also somewhat diminished in consistence, and presented numerous red spots, due to divided vessels; the corpus callosum was unaffected. On opening the lateral ventricles, the fornix, corpora striata, and optic thalami appeared equally healthy on both sides; but when the incisions were carried deeper, so as to expose the extraventricular nucleus of the corpus striatum of the left side, this part was found very much softened, and contrasted remarkably with the corresponding part on the right side; there was no trace of blood extravasation.

Finding the disease of the brain limited to the parts supplied by the middle cerebral artery, this vessel, which hitherto lay concealed in the fissure of Sylvius, was next examined; at its commencement a small clot was found completely blocking it up, and extending a short distance into the left anterior cerebral artery. This clot was firm, of a dark red colour, and moderately adherent to the wall of the vessel, from which however, it could be separated with facility; the vessel appeared to be quite healthy.

The lungs were quite healthy, and free from adhesions. The heart was firmly contracted; the left ventricle empty; the right ventricle contained a small clot; the valves were healthy; the kidneys were congested and lobulated, but there was no evidence of disease of their structure.

The brain was examined microscopically; a small portion of a softened convolution from the left side, and a corresponding portion of a healthy convolution from the right side, were examined comparatively. On the *right* side nerve cells, numerous varicose nerve tubes, and the usual molecular *debris*, were seen. The capillaries were, for the most part, healthy; but some of them, as well as the small arteries, showed a few fat granules in their walls; these were most frequent at the points of bifurcation of the vessels. On the *left* side the large stellate nerve cells were very distinct, being opaque and granular; numerous spherical granule cells were also seen. The nerve fibres were more scanty, and never of such length as in the healthy side. The blood vessels were much more fatty here than on the right side; the capillaries also were larger, contained

more blood, and in some places were slightly varicose; there were no blood crystals, nor any evidence of extravasation.

Dr. Banks observed that the pathological appearances which he laid before the society were of interest, as bearing on the subject of loss of speech in connexion with lesions of the brain. Since the promulgation of Bouilland's views with reference to the part of the brain which presides over speech, much attention had been devoted to the investigation. Bouilland believed that the faculty of speech depended on integrity of the anterior lobes.

Dax localized the faculty exclusively in the left hemisphere; but of late the startling hypothesis had been put forward by Broca (supported by twenty autopsies) that it resides in the posterior part of the third frontal convolution at the left side. The observation of many cases has lent a certain amount of support to the idea that, at least, the lesions of the left side of the brain seemed to be coincident with loss of speech.

Charcot supplied many observations in confirmation of Broca's views, but he also recorded an example of aphasia without lesion of the third frontal convolution. In the *Clinical Reports* of the London Hospital, Dr. Hughlings Jackson has published a most interesting series of cases of hemiplegia of the right side associated with loss of speech and valvular disease of the heart. In thirty-one cases in which hemiplegia with loss of speech existed, he believes that the middle cerebral artery was plugged. The case which is now submitted to the society supports, in a general way, the views of Broca and the opinion of Dr. Hughlings Jackson as to the condition of the left middle cerebral artery in cases of hemiplegia of the right side, with aphasia.—*February 24, 1866.*

Hepatic Abscess.—DR. DUNCAN said, that the rarity of hepatic abscess in this climate induced him to lay the present specimen before the society. It was taken from a man, a coppersmith by trade, 35 years of age, and reported to be of temperate habits. On the 22nd of January, 1866, he was seized with a rigor, followed by other symptoms of fever, of a sufficiently alarming character to induce him to enter the Adelaide Hospital. He was admitted on the 11th of February, nearly three weeks after his illness began. He (Dr. Duncan) saw him the next day, and, from the symptoms then present, made up his mind that it was not a case of fever. In the first place, it was not typhoid fever, because there was no eruption on the abdomen, no pain or gurgling in the ileo-cecal region, no diarrhea, nor had the alvine evacuations the character peculiar to that affection; there was tenderness, indeed, on pressing the upper portion of the abdomen, but it was distinctly limited to the right hypochondriac and epigastric regions; and the only symptom to suggest the idea of its being typhoid fever was the occurrence of two rose spots on the upper part of the back, but in this situation such an occurrence could

scarcely be considered pathognomonic. Neither was it typhus; the man was in the twenty-first day of his illness, and, if the disease were typhus, we should have had a group of symptoms that did not exist in this case; he had neither the peculiar aspect, the dry brown tongue, the sordes, nor the muttering delirium that characterize typhus. The symptoms he presented were these:—A fulness below the ribs on the right side, extending into the epigastrium, which was painful and tender to the touch; a dusky whitish complexion, as if the liver were not acting properly; a distinctly yellow hue of the conjunctivæ; the feces solid, and of a drab colour; and the urine of a dirty orange tint. These appearances led him to pronounce the case one of acute hepatitis. Throughout the progress of the case there were other symptoms which clearly showed that it was not one of ordinary fever; his intellect was perfectly clear to the last, and he was greatly exhausted by heavy perspirations, which prevented him from sleeping. Dr. D. did not consider it necessary to detail the particulars of the treatment; but he said that on the 17th February there was, apparently, a considerable improvement in the man's condition. The tumour was reduced in size, he ceased to complain of pain, and there was less tenderness on pressure. On the 18th diarrhea occurred, which was followed on the 20th by a distinct attack of peritonitis in a subacute form. Then, for the first time, he complained of pain in the lower part of the abdomen, particularly over the pubes, with general tenderness over the entire extent of the surface, and slight effusion into the cavity of the peritoneum. On the following day vomiting and hiccough set in, and he died on the 23rd.

He (Dr. Duncan) looked forward to the autopsy with considerable interest; for the occurrence of peritonitis after thirty days of fever suggested the possibility of the disease having been really typhoid fever after all, with inflammation and ulceration of Peyer's glands, and consequent perforation of the intestine. He was not, however, disposed to adopt this view, because of the absence of the proper symptoms of enteric fever throughout the illness, and because the peritonitis did not set in with the suddenness and intensity that usually characterize it when produced in this way. What, then, was the cause of the phenomena? Two solutions presented themselves to his mind—one, that the inflammation of the liver had spread by contiguity of texture to the peritoneum, giving rise to the new series of symptoms which terminated in death; the other, that the disease had been a form of peritonitis all through, but originally of a local and limited character, disturbing the functions of the liver in the first instance, by proximity of position, and extending afterwards to the rest of the membrane, thereby giving rise to the secondary train of symptoms which revealed its true nature. Against this latter hypothesis was to be set the rarity of such an occurrence in the peritoneum, though it is well known that it has been sometimes met with

in a chronic form, and the general history of the case seemed to tally more with the former supposition.

On the *post mortem* examination there was found a considerable collection of fluid, of a yellow colour, with flakes of lymph floating through it, and also adhering to the inflamed membrane, particularly about the jejunum and duodenum. The closest examination failed to discover any evidence of irritation or ulceration about the glands, or any laceration. The liver was somewhat enlarged; and, on making a section, pus, well formed and healthy, exuded freely from the biliary ducts, while several distinct abscesses were formed in different parts of its structure, particularly one large one, occupying nearly the whole of the lobulus Spigelii. The gall bladder was rather contracted in size, and contained a mixture of pus and bile. There was no ulceration to be detected in the gall bladder, though it was carefully sought for. At first Dr. Duncan was inclined to look upon the result of this examination as an instance of what is certainly very rare in pathology—idiopathic, primary abscess of the liver, because the most careful examination failed to discover any traces of ulceration or inflammation either in the alimentary tract, or in the parts where primary abscesses connected with this affection were usually found. A second examination that morning, had, however, brought to light a small strumous abscess in the mesentery, which might possibly be the source of hepatic contamination. Still there were difficulties, whichever view was adopted. It was scarcely to be believed that a small strumous abscess in this situation should have been attended with constitutional symptoms of so severe a type; and, on the other hand, the liver did not exhibit that hyperemic congestion which we should surely have expected to find had the case been one of acute inflammation, proceeding in due course to the formation of abscess.—*March 3, 1866.*

Tumour of the Uterus.—DR. BENNETT showed a specimen of fibrous tumour of the uterus, and also a drawing of the tumour from which the accompanying woodcut had been executed. The specimen was taken from the body of a woman of middle age, brought into the dissecting room of Trinity College School.

As the body lay on the table the abdomen presented very much the appearance seen in the middle period of pregnancy; but, on being handled, the tumour causing the enlargement, while evidently uterine, was much more firm in feel than a pregnant uterus. There were present two symptoms of pregnancy, independent of the tumour itself, which attracted attention—a brown marking of the middle line of the abdomen, chiefly over the tumour, and considerable enlargement of the breasts from accumulation and secretion of milk.

It is worthy of remark that, although the brown mark was the same

as that which is seen in pregnancy, it did not form an umbilical areola, a point which the late Professor Montgomery had noticed when discussing the value of these marks as signs of pregnancy. From the consideration of these appearances, and the external examination of the tumour, it was concluded that the case had been either one of pregnancy, or of recent delivery with enlargement of the uterus from inflammation—or, lastly, of fibrous tumour; this last opinion was proved to be the true one by vaginal examination and by the dissection. The finger could be introduced within the os uteri, and came in contact with the apex of the tumour.

On opening the abdomen the uterus was found to be free from adhesion to the adjoining viscera, except posteriorly, where the ovaries were abnormally adherent to it, the connexions being of old standing. Its surface presented nothing remarkable, except some few fibrous tumours in the sub-peritoneal tissue, one of which was seen in the drawing.

On opening the uterine cavity, by raising the anterior wall, the tumour was exposed to the extent shown in the drawing; nearly one half of its entire length projected into and filled the cavity, while the remainder involved the entire fundus. A probe passed through the Fallopian tube, entered into the upper angle of the incision, and so marked this part on either side as the original limit of the fundus; at this level the mucous membrane was reflected on the tumour all around.

It was evident that a tumour having so extensive an attachment, could not have been removed during life, unless the uterus were also removed with it. The pointed extremity of the tumour, lying in the cervix uteri, was covered by loose and very vascular mucous membrane, and on either side of its free portion there hung similar processes of the membrane.

Some clots of blood in the vagina and uterus showed that there had been a small amount of hemorrhage immediately before death. The cervix uteri showed the markings called arbor vitæ so distinct and perfect that it is probable that the uterus had never been pregnant. The tumour was composed of the usual form of fibrous tissue seen in these cases—narrow fibres with abundant nuclei. The case was of interest, as showing how such tumours could simulate pregnancy, and also for the great size and mode of connexion of the tumour, which, with the uterus, weighed four and a half pounds.—*March 3rd, 1866.*

Staphyloma.—DR. PURSER exhibited drawings illustrating some unusual points in the pathological anatomy of the cornea. The drawings were taken from two specimens of staphyloma recently removed, in St. Mark's Hospital, by Sir Wm. Wilde and Dr. Wilson. In one case the disease involved only part of the cornea, and had resulted from small-pox ulceration; in the other the entire cornea was protruded, the staphyloma being the result of long-continued strumous ophthalmia. On vertical section the following were the appearances seen:—

The epithelium was greatly thickened, and consisted manifestly of two layers—an outer horny layer, in which the nuclei of the cells had disappeared, and which easily separated from the inner or mucous layer, which consisted of well-formed nucleated cells, those nearest the cornea being oval, their long axis placed perpendicularly to the surface. The line of junction between the epithelium and proper cornea was very irregular. In some places the cornea sent papillary processes outwards into the epithelial layer; in others the epithelium dipped down into the cornea. These elevations and depressions were very irregular in size and shape. In some preparations portions of epithelium were seen completely cut off from the remainder of the cells, and lying imbedded in the corneal substance. It is probable, however, that these were connected with the surface by a narrow neck,

through which the line of section did not pass. These appearances were very generally seen over the surface of the staphylomata, but unequally developed in different parts. The anterior elastic lamina was nowhere visible. The corneal cells were much altered, having undergone a very great increase both in number and size. Near the surface they were comparatively small, and lay very irregularly, so that here the laminated appearance of the cornea was quite lost. Deeper they were



much larger, and in some places were undergoing a development of fibrous tissue, the change being produced by a proliferation of the nuclei, which, assuming a fusiform shape, ultimately were converted into fibres, so that from a single cell was produced a mass of fibrous tissue,

preserving, more or less, the general shape of the cell from which it had originated. In all cases the cells communicated freely with each other by their branching processes. The inter-cellular substance was



transparent and homogeneous, and showed no indication of the change into fibrous tissue which has been described by Mr. Bowman and Mr. Richardson. The posterior elastic lamina was in general absent, but in some places still persisted. To the posterior surface of most of the sections masses of black pigment, derived from the adherent iris, were attached. With regard to the irregular line of junction between the cornea and epithelium little information is to be derived from authors. Mr. Bowman, in his *Lectures on the Anatomy of the Parts Concerned in the Operations on the Eye* (page 89) gives a drawing of a corneal opacity from an ox, in which a somewhat similar appearance is seen. It is must mentioned by any other writer consulted by Dr. Purser. An appearance very similar to that presented by the corneal cells is shown in Wedl's *Atlas der Pathologischen Histologie des Auges* (Plate I, Fig. 6), from the cornea of a rabbit in whom an artificial keratitis had been induced.—March 10, 1866.

Laceration of the Duodenum.—DR. SYMES said that on Wednesday evening last a young man walked into Steevens' Hospital complaining of an injury received under the following circumstances:—Some three or four hours previously he was engaged in weighing himself, and the attachment of the iron beam gave way, the beam falling and impinging on his abdomen with violence. He suffered considerable pain and a constant desire to pass water, being disturbed every five minutes endeavouring to empty his bladder. While he (Dr. Symes) was in the ward next morning, the man suddenly died.

On examining the body, twenty-four hours after death, not the slightest trace of any injury to the integument covering the abdomen was to be seen. On opening the peritoneum it was found that peritonitis was general. There was a general effusion of serum into the cavity; the intestines were matted together with flakes of lymph, and presented every shade of vascularity from bright scarlet to purple. The bladder was contracted and empty, but uninjured. There was a laceration of the duodenum, extending to a portion only of the calibre of the tube.

That day week he had the honour of laying before the society a case in which the bladder was ruptured, and where the peritonitis was very limited in extent, just sufficient to circumscribe the extravasation of urine. In that case the opening was small and the extravasation of urine gradual. Here the opening in the intestine was large, the extravasation sudden and complete, and the peritonitis general. In the case of rupture of the bladder the patient lived twelve days; in this case only twenty hours. In many cases of peritonitis, great irritability of the bladder was present, and it was but reasonable to suppose that such should exist. As general inflammation acting from without on the walls of the stomach caused great irritability in it, so no doubt it would produce irritability of any other hollow viscus like the bladder. It was a remarkable circumstance in these cases that great internal injuries might exist without any injury of the integument covering the abdomen—not even an abrasion or ecchymosis.—*March 10, 1866.*

Tumour of the Uterus.—DR. DENHAM exhibited a uterine tumour, which had been taken from the body of a woman whom he saw for the first time so far back as 1859. She then complained of increased menstrual discharges, which in a short time became so excessive as to weaken her very much, and she was induced to enter the Rotundo Hospital under the care of Dr. M'Clintock. The tumour was situated midway between the symphysis pubis and the umbilicus. The os was completely closed. Beyond general debility she had little to complain of; her general health remained remarkably good. This was in 1860. She left the hospital, but returned in the month of July, 1861, greatly emaciated, anemic in appearance, with rapid pulse, and great general weakness, evidently arising from the loss of blood which she had sustained. Dr. M'Clintock said the os dilated freely, appearing almost like that of a woman in labour, and within the os was the tumour. He endeavoured to remove a portion of it, and made two incisions with a pair of scissors into the os uteri. On three different occasions during Dr. M'Clintock's mastership, and again when he (Dr. Denham) became master of the hospital, they endeavoured to remove portions of this tumour, and with considerable success. The os dilated considerably with very little trouble.

The only advantage derived from these successive operations was, that the hemorrhages seemed to decrease considerably. There was no general disturbance until after the third and last attempt was made. After the third operation, when there was a considerable portion of the tumour removed, there was slight uterine disturbance and inflammation which rapidly subsided. She left the hospital and returned to her home. She came back to the hospital again, with her constitution greatly shattered, suffering under rheumatism; her hands and feet enlarged so that she could not follow any occupation or work. At her urgent request they again endeavoured to remove a portion of the tumour. With the assistance of Dr. M'Clintock he made a crucial incision with a fine scalpel. This was followed by considerable hemorrhage; in fact before any incision was made the mere manipulation gave rise to hemorrhage. This was on Monday; symptoms of inflammation set in, and she died on the following Saturday.

On opening the abdomen they found some purulent matter on the anterior wall of the uterus.

The tumour, which resembled a lobulated kidney, was found to fill up the entire cavity of the uterus, the enormous size of the base showing how hopeless it would have been to attempt its entire removal. There was very little appearance of inflammation where the incisions were made. The tumour was of a dense, fibrous structure, with a few ossific deposits in its interior. In a late publication by Dr. Marion Sims he gave a drawing of this case. He was in the hospital on the occasion of one of the operations; and it was a strange thing that he described the tumour as about the size of a pigeon's egg, although, at the time he saw the woman, it extended upwards half way between the umbilicus and the pubes.—*March 10, 1866.*

Fracture of the Pelvis—Rupture of the Bladder.—DR. FLEMING detailed the following case, illustrative of a specimen of fracture of the pelvis, which he laid before the society:—

A labouring man, aged sixty, was employed in undermining one of the side walls of a house in the vicinity of the Richmond Hospital, when, while he was stooping forward, the wall gave way, and he was crushed beneath it. He was brought to the hospital some hours afterwards, sick and faint, groaning with agony, and totally powerless.

There was extensive ecchymosis in the sacral and pubic regions, blood was flowing from the urethra, and had distended the penis, scrotum, and left inguinal canal, so as to resemble an inguinal hernia. The slightest movement of the lower limbs gave pain, but the principal suffering was referred to the region of the bladder and to the symphysis pubis, where the tenderness was so extreme that the slightest pressure could not

be borne. The symmetry of the pelvis and of the lower extremities was normal, and the motor power and tactile sensations were tolerably perfect.

The immediate treatment had reference to the removal of the state of collapse and to the allaying of pain. Subsequently it became necessary to introduce a catheter; the urine drawn off was not tinged with blood. The lower region of the abdomen gradually became swollen and tympanitic, the ecchymosis increased in extent, and the crepitation of air was distinct under the integuments. Any attempt to change the position of the patient caused agonizing pain, nor could he endure the slightest movement of the limbs upon the pelvis. Reaction did not take place. His countenance became pallid and haggard; and he died with the symptoms of internal hemorrhage, in about forty-eight hours after the accident.

Upon examination, *post mortem*, the ecchymosis, tympanitis, and emphysema observed during the progress of the case were found to occupy the entire of the hypogastric, iliac, and inguinal regions, as well as the tract of the inguinal canal. On the left side of the symphysis pubis a distinct projection was felt, the points of the fingers sinking into a hollow along its margin. When the walls of the abdomen were perforated in the hypogastric region, a quantity of air, devoid of any fetid odour, escaped from the cavity, which was found to contain a large quantity of blood, part of which had made its way into the pelvis and into the left inguinal canal.

At the symphysis pubis there was a distinct separation of the bones, the left horizontal ramus being on a plane considerably above and behind that of the right, and a thin scale of bone being detached from it, and left firmly adherent to the intervening fibro-cartilage. There was a deep sulcus, stained with blood, between the true ligaments of the bladder, in the anterior wall of which a vertical rent was discovered, about a quarter of an inch in extent anterior to the reflexion of the peritoneum; the edges of the rupture were so closely applied to each other that it was only by the escape of bubbles of air from the bladder, when it was compressed, that the solution of continuity was discovered. The urine, of which there was about three ounces in the bladder, was unmixed with blood. The ecchymosed and lacerated state of the perineum and scrotum rendered it impossible to isolate the urethra, which was torn across in its membranous portion.

On proceeding to remove the pelvis, a partial separation of its bones, was found at each sacro-iliac synchondrosis and also a vertical fissure extending through the right side of the body of the sacrum, and traversing the sacral foramina. There was no trace of inflammatory action in the peritoneum or elsewhere.

With reference to injuries of the pelvis in general, Dr. Fleming observed that their diagnosis was frequently involved in great obscurity, and that the propriety of attempting an accurate adjustment of the

displacements consequent upon them was often extremely questionable. As

in certain cases of fracture of the ribs, mechanical compression of the thorax could not be tolerated, so likewise in fractures or other injuries of the pelvis, the agony resulting from any constrained position for adjustment was frequently extreme, and its advantages were very questionable. Too much anxiety to detect crepitus on the one hand, and on the other, to remedy accompanying deformity, may be followed by fatal consequences—such, for example, as hemorrhage, or additional injury to the contained viscera.—*March 10, 1866.*

DR. BARRON exhibited a morbid specimen showing the slow but fatal results of long-continued obstructions of the urethra. A man, aged between fifty and sixty, came into hospital on last Saturday. His symptoms were as follows:—His lips were livid; his breathing hurried; his pulse weak and failing; the scrotum and both lower extremities were cedematous, and covered with an eczematous eruption, discharging a thin fluid. His dyspnea was so severe that he could scarcely lie down, and he was continually straining to pass water. He stated that these symptoms had existed for a long time, and had become gradually worse, compelling him to seek for admission into the hospital.

He stated that he had been subject to stricture for more than twenty years. Sixteen years ago he had suffered a severe attack of total retention. He was then under the care of Mr. Wilmot, who performed the perineal section through the stricture. Immediate relief followed the operation, and after the wound healed he could pass No. 6 catheter, which he continued to do, although suffering more or less from the obstruction caused by the stricture; but he was able still to pass the

instrument. Latterly he suffered from frequency of passing water. This increased, and then the other symptoms which had been mentioned came on. They found that No. 5 catheter easily entered the bladder, but the orifice of the instrument became so clogged with mucus that not more than three ounces of urine could be drawn off; the urine was mixed with blood, and was highly albuminous. The action of the heart was feeble, and he had cough. He died on Tuesday evening.

On examination they found a well marked example of stricture of the urethra, but presenting some features of peculiar interest. In the first place it was situated a little in front of the bulb, and was formed by a mass of lymph extending for fully two inches along the urethra. On examining the canal behind, they did not find much dilatation, which accorded with Guthrie's observation that considerable dilatation was not generally to be expected, even in cases of aggravated stricture. The prostate gland was healthy; the walls of the bladder were enormously thickened; the mucous membrane was of a dark greenish colour, deeply coated with phosphate of lime and tenacious mucus; the ureters were largely dilated; the left kidney resembled clotted liver; the calices were very much dilated, and the structure of the gland was infiltrated with a hard substance. The right kidney presented a somewhat different appearance, but its calices were also dilated. Probably the mass of lymph originally deposited had been increased by the operative proceedings that became necessary when he had complete retention of urine, which would probably have destroyed the patient at the time if it had not been relieved by operation. Subsequently it produced death by other pathological changes. The thickened bladder, the inflamed mucous membrane, the dilated ureters, and, finally, the inflammatory action which prevented the kidneys from performing their functions, were produced by a *constantly existing*, though slight, obstruction in the urethra—for a No. 6 catheter was constantly introduced, notwithstanding which the fatal effects of stricture gradually developed themselves.—*March 17, 1866.*

Amyloid Degeneration of the Liver.—DR. PURSER made the following communication:—The specimens which I lay before the Society consist of the liver and one of the kidneys of a man who died in the South Dublin Union Workhouse. They were given to me for microscopic examination by Dr. Jennings, under whose care the patient was, and at whose request I now exhibit the parts.

The history of the case is not very perfect. The patient had long suffered from constitutional syphilis, affecting the bones. When he came under Dr. Jennings' care he was affected by uncontrollable diarrhea and vomiting, under which he sank.

On opening the body, the kidneys at once attracted notice from their great size; they weighed, in the recent state, 1 lb. 9 oz. each. The

capsule separated readily; the surface of the kidney was smooth, of a yellowish-grey colour, and presented in some spots the usual stellate vascularity. On section, the increase of bulk was seen to involve both cortical and medullary substances, but chiefly the former. The cut surface had a homogeneous appearance; and, when viewed obliquely, the Malpighian bodies could be seen as shining translucent granules. The cortex was remarkably bloodless, but the vessels of the pyramids contained blood in some quantity. Very little juice could be obtained from the cut surface by scraping or pressure. The whole organ had a tough, resistant feel, like Indian rubber.

On examining thin sections with the microscope, the increase in bulk was seen to be due, for the most part, to a great increase in the inter-tubular tissue of the gland, which, as a nucleated connective tissue, separated the tubes and vessels from each other in a most remarkable manner. This was seen in both cortex and medulla, but was most remarkable in the former, both from its absolutely greater quantity, and from the small amount of connective tissue which is normally present in this part. The Malpighian capsules appeared as thick laminated sacs enclosing the capillary tufts. The arteries and Malpighian capillaries were affected by amyloid degeneration in the most advanced degree. Their walls were thickened and shining, and gave the characteristic red reaction with iodine. In some cases it appeared as if the vasa efferentia and the inter-tubular capillaries were similarly diseased; but, as the attempts to inject the kidney failed, owing to the organ's having been cut into before it was sent to me, I do not wish to speak with confidence on this point. The vasa recta were universally degenerate; the walls of the convoluted tubes were thickened. The epithelium in some places adhered to the walls, leaving a central canal; this epithelium had a more homogenous and resistant aspect than normal, as if infiltrated by some transparent exudation. Fragments of it easily separated, and floated in great numbers in the fluid of the preparation, presenting their thin edges to view, and appearing as crescentic bodies, in many cases not unlike the fibre cells of the spleen, and disclosing their true nature only when they rolled over and presented their flat surfaces to view. In some places the nuclei persisted, but for the most part they were withered or absent; in other places the tubes were distended by a granular material, probably of fatty nature. In others the tubes contained a translucent and homogeneous substance, forming casts of the tubes, often of considerable length. This substance had very much the appearance of the morbid deposit in the walls of the degenerate blood vessels, but did not stain with iodine. It offered a remarkable resistance to reagents, being unaffected by dilute acetic or hydrochloric acid, or dilute solutions of caustic alkalies, ether, and boiling alcohol. It was found for the most part in the convoluted tubes; and when present in

the straight tubes, had evidently been washed down into them from smaller tubes, for it lay loose in their cavity.

These casts are very brittle, breaking easily transversely, with a sharp fracture. They are very different from the usual *hyaline* or *fibrinous* casts found in the urine, in the ordinary chronic forms of Bright's disease. Their presence in the tubes is, so far as I have seen, constant in all cases of advanced amyloid degeneration; and from their presence in urine, I have been enabled to diagnose amyloid degeneration without having seen the patient—a diagnosis subsequently confirmed by *post mortem examination*. Of the composition of these casts it is impossible to speak with certainty. They are probably of a fibrinous or albuminous nature, and possibly a further stage of the exudation which has been already described as infiltrating the epithelium of the convoluted tubes. The straight tubes did not present any remarkable morbid appearance. In no instance could I satisfy myself that the amyloid degeneration, as evidenced by the iodine reaction, had extended beyond the blood vessels.

The liver was enlarged, heavy, smooth on the surface, tough, and bloodless. In it also amyloid degeneration was found involving only the ramifications of the hepatic artery, and a few of the cells in the outer parts of the lobules; in other places the cells contained much fat.

Other organs were not examined.—*March 24, 1866.*

Diseases of the Mitral, Aortic, and Tricuspid Valves.—DR. JENNINGS brought the following case before the society:—Michael Toole, aged forty years, was admitted into hospital on 13th February, 1866, labouring under intense dyspnea, incessant cough, attended with copious expectoration, general oedema, and anasarca. His health, according to his own statement, had been tolerably good until the latter part of the past year, when cough, difficulty of breathing, and pain in his right leg, compelled him to seek relief in one of the city hospitals, where he had continued under treatment until a few days previous to his admission into the South Union. On percussion intense dulness was found to exist on both sides of the chest in front and behind, except for a space of about three inches below the clavicles, where puerile respiration, interspersed with occasional loud sonorous râles, was audible. At the apex of the heart, at both sides of the left nipple, both below and above its level, were heard two distinct systolic bruits, short and abrupt; while over the area of the aortic orifice existed a double murmur, totally different in character, being much rougher and harsher. The first of these latter sounds was so intense and prolonged as almost to efface the second, which, however, became perceptible on careful examination, and was loudly audible over the entire anterior, posterior, and lateral regions of the chest; being, in fact, so diffused as effectually to mask all cardiac rhythm, and any respiratory sounds. Strong pulsation was observed in both carotid and subclavian

veins, and slight throbbing in the left radial and ulnar arteries. The pulse was 120, exceedingly weak, readily obliterated by pressure, yet regular. During the few following days, under the free use of stimulants, extensive blistering, diuretics, &c., the cough, dyspnea, pulmonary congestion, and general distress were reduced; and on the 17th, the cardiac rhythm, for the first time, became perceptible, and the different bruits—especially the aortic systolic—less violent, and for this reason more decidedly distinguishable. This rally, however, proved only very temporary; former symptoms soon returned, and in an aggravated form; the anasarca (which had almost disappeared) returned, and rapidly increased; and the vital powers gradually sank until the morning of the 21st, when he died, having lain in a semi-comatose state for the preceding twenty-four hours.

The diagnosis arrived at was, that each of the orifices in the vicinity of which the different bruits were audible had undergone morbid changes; that the murmur heard above and to the right side of the nipple, taken in connexion with the general venous turgescence and general œdema, indicated tricuspid regurgitation; that that heard at the left of this point, together with the pulmonic engorgement, pointed out mitral reflux, while the double basic murmur established the existence of aortic obstruction and patency. The absence, *to any great extent*, of visible arterial pulsation, so constant a symptom in this latter lesion, was satisfactorily accounted for by the fact that the quantity of blood transmitted through the systemic vessels was reduced to a minimum, in consequence of the free regurgitation taking place through the tricuspid and mitral orifices.

The correctness of this diagnosis was verified in every particular by the *post mortem* examination. The pleural cavities contained each about two pints of serum; the lungs were intensely engorged, and dark in colour, the tubes being filled with frothy mucus. The heart was globular in form, and enlarged, chiefly from eccentric hypertrophy of the left ventricle, which constituted the principal part of its bulk, though the other chambers also had undergone the same change; its tissue was soft and friable, and easily torn; the right auricle was occupied by a large fibrinous clot, which extended into the venæ cavæ; the right auriculo-ventricular orifice measured in circumference five and a half inches; the right ventricle was lined by a firm fibrinous coating; the anterior and posterior divisions of the tricuspid valves were firmly attached, the former to the free wall of the cavity, the latter to the ventricular septum, save a margin of each, of about three or four lines in depth. Both the left chambers were occupied partly by sanguineous, and partly by fibrinous clots; the left auriculo-ventricular orifice was, like the right, vastly dilated, measuring in circumference five and a half inches; the posterior flap of the mitral valve was also firmly adherent to the ventricular wall, while the anterior was covered on its aortic surface with soft vegetations. The aortic semi-lunar valves were thickened, puckered, and occupied by

morbid deposits ; and the vessel itself was covered throughout its entire arch by atheromatous and osseous scales.

Disease of the Mitral Valves.—DR. JENNINGS exhibited the recent specimen in this case, which was that of a woman, aged forty, a cook, who had during the past two years suffered from dyspnea, palpitation, and cardiac oppression—symptoms which had latterly become greatly aggravated. She was admitted into hospital on the 19th February last. The difficulty of breathing at this time had merged into constant orthopnea ; her face was bloated and livid ; there was considerable anasarca and ascites, also frequent irritating cough, and coldness of the general surface. The heart's action was exceedingly rapid, irregular, and fluttering, and its double beat undistinguishable ; the external jugular veins were prominent ; and in them, as also in the subclavian veins, a tremulous motion, very like that seen in a strongly sucking leech, was perceptible on close observation. Repeated examinations failed to detect the slightest arterial pulsation at the wrists, or even along the arms, nor could the faintest bruit be heard in the precordial region ; both sides of the chest were extremely dull. On the 4th of March she had an attack of hemoptysis, which reappeared on one or two subsequent occasions.

Here were two distinct groups of symptoms, strongly contrasted, indicating lesions of opposite sides of the heart. The pulmonic congestion and hemoptysis, the rapidity and fluttering nature of the heart's action, taken in connexion with the failure of the arterial pulse, spoke of mitral obstruction—the very absence of cardiac murmur telling how advanced was the disease, how complete the occlusion ; while the cerebral congestion, the venous turgescence and pulsation, the extreme anasarca and ascites, could only be accounted for by the existence of regurgitant disease of the tricuspid aperture. A few days prior to her decease she suffered a severe convulsive attack, described by the attendants as distinctly epileptic in character ; and on the 9th, having lain in a state of semi-coma for the preceding two days, she was released by death from her protracted sufferings.

The *post mortem* examination disclosed extensive serous infiltration of the cellular tissue, as also effusion into the abdominal and right pleural cavities, which latter was completely filled. Both lungs displayed fine specimens of apoplectic extravasation—the right of the diffused, the left of the circumscribed variety.

On opening the pericardium, the heart was found to be enlarged—the chambers which had undergone the principal change being the two auricles, and the left ventricle, the walls of the latter being greatly thickened, and its cavity proportionably reduced in capacity. Both auricles were dilated, and filled with dark coagula ; the chamber which was undoubtedly least altered in every respect was the right ventricle, which would not under ordinary circumstances have attracted any special notice.

The mitral orifice, viewed either from the auricle or ventricle, presented the appearance of a narrow semilunar fissure; all trace of valvular structure had disappeared, both divisions being converted into a thick firm tissue, almost as rigid as cartilage. So firm and close was their apposition, that the introduction between them even of the handle of a scalpel required considerable force; and from their structure and peculiar arrangement, they must, during life, have permitted merely a trickling current of blood to pass. The chordæ tendineæ were much thickened, as were also the muscular columns connected with them.

The aortic aperture was much contracted, barely admitting the tip of the index finger; its valves were indurated, thickened, and puckered. The tricuspid valves likewise were diseased, and occupied by a morbid deposit, the left flap being moreover very cribriform: all were contracted in outline, and connected together, so as to form a funnel-shaped process, terminating in a circular aperture. The opening of the pulmonary artery, and its valves, were healthy, nor were the pulmonic veins visibly increased in calibre.—*March 24, 1866.*

Softening of the Brain.—DR. HAYDEN said, that the brain, heart, and portion of the lung, which he now laid before the Society, were taken from the body of a female, aged forty-seven, who was admitted to the Mater Misericordiæ Hospital on the 17th of last month, and died on the 24th. It seems that this woman, whose occupation was that of a seamstress, had enjoyed tolerably good health, with the exception of a few attacks of rheumatism, up to the 27th of last December; on that night she went to bed in her usual health; and her husband, on the following morning, found her paralyzed on the right side, and unable to speak. The woman remained under the care of a medical man in this city from that time until the 17th of March, on which day she was admitted into hospital under his care, in the following condition:—There was complete paralysis of motion of the right upper extremity, and partial of the corresponding lower extremity. The woman was able to draw the leg up in bed, and to stretch it out; and it was reported to him, that on her admission she walked into the ward without much assistance, by dragging the right leg after her. There was also a partial paralysis of the right side of the face; the left angle of the mouth was drawn towards the left ear. She could protrude her tongue; it deviated slightly to the right side, but she was able to move it to the left; she was also able to close both eyes, and to throw the skin of the forehead into transverse wrinkles; she was incapable of uttering a word beyond a monosyllable. She seemed perfectly intelligent; the pupils were normal and equal; the pulse 160, irregular and intermitting, as was also the heart's action; the sounds of the organ were loud, ringing, and extensively diffused over the anterior surface of the chest. The patient could take food, both solid and liquid,

and swallow it well. The history of the case; the fact that there was manifestly disease of the heart, even though there was no murmur; the suddenness of the occurrence of the symptoms; the paralysis of this special form, namely, right hemiplegia with loss of speech, led him to the conclusion that the case was one of lesion of the upper part of the left motor tract, and likewise of the left anterior lobe of the brain, supposed to be the seat of language. It was probable, under the circumstances, that these alterations were due to the impaction of an embolus detached from the heart, carried upwards, and arrested in the left middle cerebral artery. The latter portion of this diagnosis was only probable or conjectural; the former, manifestly well-founded.

From the 17th of March till the 21st there was very little change. On the 21st the clinical assistant noticed that the woman endeavoured to convey that she felt pain in the head; he put his hand on her forehead, guided by her left hand, of which she had perfect use, and he was struck by the fact that on the right side of the forehead the temperature was greater than on the left, and that there was also a distinct throbbing on that side; the patient likewise complained of considerable tenderness on the right side of the forehead.

As regards the faculty of speech, there was ample opportunity of testing it. He frequently put questions to her; she was unable to answer, except by the monosyllables, "yes," and "no;" he asked her name, but she could not tell; he went over a long list of names, and when the wrong name was mentioned, she invariably said, "No, no;" but, when the right name was pronounced, she immediately said, "Yes, yes, yes;" and seemed quite pleased at the discovery he had made. On one occasion she made use of the word "six," in answer to a question as to the duration of her illness. She intended to have said "six weeks," but broke down after the utterance of the word "six." She always, by signs with the left hand, was able to communicate some vague general notion of her suffering, which was chiefly located in the head. She was perfectly intelligent, recognized her husband when he came into the ward, and endeavoured to communicate, through him, the sense of gratitude she felt for the attention that had been paid her.

On the 23rd of March her pulse had become imperceptible in both wrists; the heart's action was extremely feeble; the backs of the hands and feet had become livid, and remarkably cold. Now, for the first time, the bladder and rectum were evacuated involuntarily. She was able to swallow fluids and liquid food.

On the 24th neither the heart nor pulse was felt; she was unable to swallow; she moaned piteously, as if in pain; was unable to move any part of the body except the left leg and arm; and on the evening of that day she died, having ceased to moan some time before, or attempt to express her feelings or sufferings; there was no convulsion before death, which occurred very quietly.

On the following morning a *post mortem* examination was made. On the removal of the brain, it was found that the left anterior lobe was altered considerably; and a drawing, made by one of the pupils in the hospital, showed very clearly and faithfully the contrast between the two lobes. From this drawing the annexed woodcut was made by Mr. Oldham. The convolutions and sulci on the inferior surface of the left anterior lobe were obliterated; the left olfactory nerve and bulb were on the free surface of the brain, not imbedded in a sulcus, as in the healthy organ, and were semi-fluid in consistence; the former had assumed a cylindroidal figure, having lost its angles and facets. He could not, however, in consequence of the inability of the woman to express her feelings fully, ascertain if there was any impairment of the sense of smell. The left anterior lobe was of a light yellow colour, and very soft, almost pulpy.

A Softened olfactory nerve and bulb of left side raised out of the corresponding sulcus by obliteration of the latter, resulting from diffidence of the adjacent cerebral substance. It will be likewise observed, that by falling in of the left anterior lobe, the olfactory nerve on that side is removed to a greater distance from the great longitudinal fissure than is the case on the opposite side.

B Situation of obliterated orbital convolutions.

C Situation of obliterated marginal convolution.

The disproportion in volume between the anterior lobes of the right and left side is well represented in the woodcut, as was likewise the contrast in the colour of these lobes in the original drawing.

On examining the anterior lobe more carefully, he found that not only were the convolutions on its inferior surface obliterated, but the second and third frontal convolutions were also effaced, the sulcus between them being filled up, while the first frontal convolution remained in its integrity. It would be seen that the island of Reil, in the fissure of Sylvius, had undergone a corresponding change—it was completely obliterated. He examined the left middle cerebral artery with the expectation of finding an embolus, but did not succeed in doing so; there was no embolus in this vessel, or in any of its branches, nor any alteration in the texture of the vessels themselves. That day he examined carefully a portion of the left anterior lobe microscopically; it was of the consistence of cream; the cineritious substance consisted of fat globules of various sizes, large granular cells, compound granular corpuscles, many crystals of margarine, and a few of hematine. The white or medullary substance was composed of the same elements, with the addition of beaded nerve fibres, and a few plates of cholesterine. On making a horizontal section of both hemispheres of the cerebrum, about the anterior three-fourths of the left corpus striatum were found entirely broken down, of the consistence of thick cream, and to have completely lost their identity on the surface of the section. The extra-ventricular nucleus had disappeared; but a small portion of the intra-ventricular, about the size of a pea, remained in the small extremity of the corpus striatum, and in the neighbourhood of this the brain substance was firm. The optic thalamus was unaltered, as was likewise the entire motor tract on the right side. There was no extravasated blood, nor any evidence of a former apoplectic effusion.

The heart presented features of some interest; on the right side there was no alteration, but on the left there was a considerable contraction of the mitral orifice; this was due to cohesion of the two segments of the mitral valve, rather than to thickening; the chordæ tendineæ were very slender. The left auricle was somewhat hypertrophied; the upper portion of it was one-fourth of an inch thick, and the centre, one-eighth of an inch; the thickness of the walls of the left ventricle was at the middle half an inch, and at the apex a quarter of an inch; the mitral orifice barely admitted the tip of the index finger; the aortic valves were partially diseased; their competency, however, was but slightly impaired, for, on pouring water into the aorta, it escaped very slowly into the ventricle. The condition of the heart possessed very considerable interest in this respect, that there was no murmur audible, notwithstanding the existence of mitral contraction in a high degree. He had examined the heart carefully every day, with a negative result as regarded murmur; and he was now led to the conclusion that its absence was due to the extreme feebleness of the patient; and that, if there had been more heart force, a murmur would have been detected.

The case furnished a good example of aphasia, or aphemia, due to

destruction of the left anterior frontal convolutions. This did not consist of loss of the *ideal* portion of the faculty of speech; for clearly the woman was full of ideas, knew the names of things, and was partially successful in expressing her ideas by means of monosyllables and signs; she made a correct application, by assent and dissent, of the names of persons and things. She seemed greatly annoyed when she failed to convey her meaning, got flushed in the face, and excited; and when assistance was given to her, she seemed disappointed at our failure to interpret her thoughts; but, when our efforts were successful, she was manifestly pleased. This loss of articulate speech was not due to paralysis of the organs of speech; for the woman could use the tongue, could move it to either side, and was able to grasp the drinking vessel with her lips; he concluded therefore, that it was the result of the loss of that co-ordinating power which we possess over the muscles of the organs of speech, and by means of which we are enabled to modulate these organs so as to give rise to the phenomenon of articulate language. He discovered in the upper part of the right lung a good example of pulmonary apoplexy, which, according to his experience, was generally to be found in cases of death from mitral obstruction.—*April 7, 1866.*

Obesity of the Heart.—DR. LAW exhibited an example of obesity of the heart, taken from the body of a woman, aged sixty-seven, who was brought into Sir Patrick Dun's Hospital, labouring under most urgent dyspnea, dependent on extensive emphysema of the lungs, with bronchitis. She was universally dropsical; and in point of fact, when admitted into hospital, she was *in extremis*. He understood that she had been for a long time labouring under asthma, and had occasional bronchitis, every attack causing increased distress, and at last general dropsy came on. He was anxious to show the specimen, as illustrating the mode in which emphysema of the lungs affected the heart. There was extensive eccentric hypertrophy of the left ventricle, showing that, no matter how far any obstruction might be from the organ, it was the left ventricle which was stimulated to act against it. In the right ventricle there was extremely little muscular structure remaining, but an extensive deposition of fat. It was often asserted by systematic writers on cardiac disease, that in the cases of pulmonary emphysema there was hypertrophy of the right ventricle; and that there was strong action of this portion of the organ, because its action was heard in the epigastrium. Dr. Law believed that frequently what was supposed to be the action of the right ventricle in such cases was really that of the left, the whole organ being pushed out of its natural position towards the epigastrium, and the left ventricle thus occupying the usual place of the right. Dr. Law remarked how extremely rarely he had met with examples of hypertrophy of the right ventricle; he might almost say, the only unequivocal instances were

where there was either an open foramen ovale, or an imperfect septum ventriculorum. He therefore attributed the hypertrophy in such cases to the stimulating quality of the blood, while he referred the absence of hypertrophy, even under conditions and circumstances calculated to induce it, to the venous blood, which was wanting in that property which was the proper stimulus of muscular action. Dr. Law believed that it was to this unstimulating or even sedative character of venous blood that the lungs owe their safety from the action of the heart, which might be expected to be strongly excited on every occasion of either temporary or permanent obstruction to the course of the blood through them. If the lungs had not some protection of this kind, their proximity to the heart would expose them to great and frequent danger; and nature would appear to have been less careful to provide for their safety than she was for that of the brain, which, although placed at a greater distance, she sheltered from the strong action of the heart, by the arrangement of the artery leading to it.

Dr. Law thought that the lung owed its security against the danger that it might be thought to be exposed to from the heart—1°, to the unstimulating quality of the blood in the right ventricle; 2°, to the arrangement of the vessels in the lungs, somewhat resembling a *rete mirabile*; 3°, to the imperfect tricuspid valve, which afforded a certain measure of relief.—*April 14, 1866.*

Aneurism of the Aorta.—DR. HAYDEN, submitted to the society an example of aneurism of the ascending portion of the arch of the aorta, of very considerable size. The patient was a man aged thirty-three years, and of intemperate habits. He was admitted into hospital on the 12th of February; and he stated that his health had been uninterruptedly good until he met with an accident, which consisted in his being knocked down by a cab, while under the influence of drink, two years previously. He was stunned by the blow, but recovered, as he thought, perfectly. Six weeks before admission he felt a pain in the upper part of the chest, on the right side; about three weeks subsequent to this date he observed a pulsating swelling in the same situation. When he (Dr. Hayden) saw him on the 12th of February, he was pale-looking, well nourished, apparently in good health, slept well, and lying down without embarrassment of breathing. His pulse was 84, perfectly regular, of moderate volume, and equal on the two sides. The pupils were normal and equal. He complained of pain in the chest, extending to the shoulder. On examining the man's chest, Dr. Hayden was struck by the presence of a considerable prominence at the upper and anterior part of the right side; it was semi-globular in figure, and projected about an inch and a half in advance of the opposite side, closely resembling the female breast, elevated somewhat above its natural situation. Over this prominence, which

extended vertically from half an inch below the clavicle nearly to the nipple, and from the right margin of the sternum horizontally outwards, measuring in both diameters about three inches, dulness was complete; there was to the same extent absence of respiratory sound, and the hand placed over it experienced a double shock—the first strong, and coincident with the cardiac impulse, or a little posterior in time; and the second, distending or elevating the tumour less, and apparently a back-stroke, received by it in the act of recoiling. In the same situation a double sound was heard; both sounds were remarkably clear, and synchronous with those of the heart, and entirely free from murmur; the impulse was stronger in the outer than in the inner portion of the tumour, and was here diastolic. Elsewhere over the right side, and over the entire of the left, percussion sound and respiration were normal. The apex beat was in the sixth intercostal space, half an inch externally to the nipple; there was no increase of precordial dulness, and no cardiac fremitus. The first sound, as heard over the apex, was somewhat soft and prolonged, but without distinct murmur; over the base, however, a sharp “whiffing” murmur was heard to accompany the first sound; it was loudest in the area of the pulmonary artery, viz., at the sternal extremity of the second left costal cartilage, but was universally diffused over the left side anteriorly; it was audible, however, only during the latter part of expiration, and the early part of inspiration, being entirely inaudible at the acme of inspiration, and in the early period of expiration, and likewise to the right of the mesial line. Posteriorly, respiration was somewhat louder on the left than on the right side; and over both sides a double sound, synchronous with those of the heart and tumour, was heard, but no murmur. No impulse was perceptible behind, nor was there anywhere tenderness on pressure.

As regarded the diagnosis, there was no difficulty. The sac of the aneurism was manifestly filled with fluid blood, for it was possible by firm pressure with the fingers to reduce the bulk of the tumour. There was neither dysphagia, nor dyspnea, even in the recumbent posture. Severe pains were occasionally complained of, extending to the right scapula, and depriving the patient of sleep; these were invariably and effectually relieved by one or two leeches applied to the part affected, and the administration of forty drops of the solution of hydrochlorate of morphia. The pulse varied from 78 to 96, and was regular; the decubitus was on the right side, and slight œdema existed, in consequence, in the right shoulder and axilla; slight pressure caused murmur in the abdominal aorta, visible pulsation, and double sound, but no murmur in the carotids.

The condition of the man underwent very little alteration from the 12th of February to the 14th March, during which time he had been taking iron; on the latter day Dr. Hayden remarked that the tumour had advanced considerably in the direction forwards and outwards; there was

œdema of the right side, extending to the forearm, where sharp pain was complained of; pulse 96; the tumour had become more solid, but action in it was strong and bounding. The iron was now stopped, and in place of it digitalis was given, and two leeches were applied in the axilla. On the 19th March the pain was so severe as to produce copious perspiration; breathing was now somewhat embarrassed, even in the sitting posture. The tumour was now much more prominent, and uniform on the surface; it projected two inches in front of the normal level.

On the 23rd of March there was dulness over the lower part of the right side, where likewise respiration was feeble, and somewhat bronchial.

March 25.—Passed a good night, and declared he felt better this morning; called for his medicine at half-past six o'clock, and whilst in the act of sitting up in bed to take it, he suddenly complained of agonizing pain in the abdomen, cried out that he was dying, became much agitated, and deadly pale, and died quietly in about thirty minutes, the tumour having ceased to pulsate from the period of the first attack of pain.

Post mortem examination—twenty-eight hours after death.—The right side, measured over the protuberant portion, exceeded the left by three inches and a half. On laying open the chest, the right pleural cavity was found filled with coagulated blood and serum, the former weighing three pounds. On removing this, an enormous tumour, as large as the head of a newborn infant, was seen to occupy the upper portion of the pleural cavity; it appeared to rise from the upper portion of the middle mediastinum; projected forwards and outwards, contracting adhesion to the anterior wall of the chest, which it protruded, and through which it projected, by erosion of the second, third, and fourth ribs.

This tumour was invested by the pleura in the greater portion of its circumference, and was unsupported save at the root, and inferiorly, where the middle lobe of the lung was attached to it; posteriorly and inferiorly to this point was a large and irregular rent, through which two fingers might be passed, and from which projected a jagged mass of partially decolourized coagulum. The right lung was compressed into the inferior posterior portion of the pleural cavity, was of a dark slate colour, and emptied of air, with the exception of the superior and middle lobes, which were adherent to the tumour, expanded upon it, and contained some air. The left lung was vascular on the surface, and partially emphysematous on the anterior margin, but otherwise healthy. The pericardium contained about two ounces of serum; the heart presented a good deal of superficial fat at the base, and a thick layer in front of the ascending portion of the arch of the aorta. On the right muscular appendix was a large "milk spot," and a similar one existed in the usual situation on the right ventricle; on the anterior and inferior surface of the left ventricle was a nodule of white adherent lymph, of the size of a small pea; the right cavities were normal; the left ventricle much thickened, and diminished in capacity. The valves were all in a healthy

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orifice of communication with the aorta. The pulmonary artery and its valves exhibited no departure from the state of health; the three great vessels arising from the arch of the aorta were unaffected; the œsophagus was not compressed or altered in its course, but both pneumogastric nerves seemed much thickened.

The following measurements of the heart were taken ninety-six hours after death:—

Thickness of walls of left ventricle.	{	Near apex, . . .	$\frac{3}{4}$ -inch.	
		Middle portion, . . .	$\frac{1}{2}$ -inch.	
		Near base, . . .	$\frac{3}{4}$ -inch.	
Dimensions of left ventricle.	{	Length of cavity from root of mitral valve to apex, . . .		2 inches.
		Width of do. in central part, measured from septum, . . .		$\frac{1}{2}$ -inch.
		Do. of do. at base, . . .		$\frac{3}{4}$ -inch.

The vena cava descendens and the venæ innominatæ had entirely escaped pressure, as had also the right bronchus, the œsophagus, and the great vessels arising from the aorta; hence there was no venous engorgement of the upper portion of the body, no dysphagia, and, where the aneurism did not intervene between the chest wall and the lung, no loss of respiratory sound. The compression of the inferior portion of the right lung was due to the sudden effusion of blood which took place half an hour before death.

The right side of the heart was normal; the left ventricle was hypertrophied, and its cavity rather diminished. Dr. H. did not see how it could be denied that this was an example of concentric hypertrophy. In the central portion, the wall of the left ventricle was thirteen-sixteenths of an inch in thickness; at the apex, and also at the base, it was three-fourths of an inch. The length of the left ventricle was two inches, and the width from the septum to the left side only half an inch, the measurement being taken when the parts were placed as nearly as possible in a natural position, and ninety-six hours after death, when *rigor mortis* must have ceased. The mitral valves were perfectly healthy, but the aortic were partially contracted. It was found on pouring water into the aorta that it returned into the left ventricle, but the return was very slow—the valve was insufficient, rather than diseased.

Some very high authorities held that concentric hypertrophy was always connected with disease of the mitral and aortic orifices. Here there was concentric hypertrophy, but no disease of the mitral orifice, and only partial disease of the aortic orifice. The question was, what had determined the thickening of the walls of the left ventricle without dilatation? The answer could be only hypothetical. His own idea was, that it was due to the presence of the aneurism in close proximity to the aortic orifice. Up to a period shortly anterior to death, there was no coagulum. The cavity would contain, when distended, perhaps two pints of blood, and the left ventricle had to fill this cavity to distention,

or rather to throw additional blood into it to the amount necessary for that purpose, before filling the other vessels, hence thickening of its walls. Then, again, the condition of the aortic orifice, although admitting of partial regurgitation, was not sufficient to determine dilatation of the left ventricle in connexion with hypertrophy caused by increased action.—*April 14, 1866.*

Disease of the Mitral and Aortic Valves; Hypertrophy of the Left Ventricle.—**DR. JENNINGS** detailed the following case:—John Martin, aged 22 years, had, about three years ago, suffered from an attack of acute rheumatism, at an early stage of which well-marked symptoms of cardiac complication manifested themselves. From this illness his recovery, notwithstanding the prompt employment of active treatment, was but very partial. The heart's action continued excited; and in a short time a single soft systolic bellows'-sound become audible over the mitral area. After the lapse of about two years, during which the original systolic bruit gradually became much louder and also distinct along the aortic arch, and the extent of cardiac dulness greatly extended, he had a second attack of rheumatic fever, which was followed by a convalescence still more tedious and imperfect than the former. The heart's rhythm became stronger, and more tumultuous, while the pulse was small, rapid, and irregular, and the face livid and bloated. The slightest exertion induced distressing palpitation, and during the last three months of his life he suffered from constant orthopnea and extreme anasarca. At no period could inter-scapular murmur be distinguished; and during the last month the complete disappearance of morbid sounds would have prevented any person who had not seen the case at an earlier stage from diagnosing the precise form of cardiac lesion under which the patient was suffering.

Autopsy.—On opening the thorax, the lungs were found intensely congested, and the posterior part of the right was occupied by apoplectic extravasation. The heart was enormously enlarged, the increase being principally confined to the left ventricle, which constituted fully half the size of the entire organ, its cavity being capable of containing a large goose egg. The lining membrane was thickened and glistening; its muscular columns were greatly hypertrophied, and the chordæ tendineæ singularly rigid. Both the mitral and aortic apertures were considerably contracted, admitting with difficulty the extremity of the index finger. The mitral valves were much thickened; and those of the aorta were contracted and cartilaginous, and presented a central deficiency, triangular in outline, permitting the easy passage of the tip of the little finger. The calibre of the aorta was greatly reduced, the descending portion of the arch being scarcely one-half inch in diameter. The tricuspid and pulmonic apertures and valves were healthy.

Dr. Jennings said, that several observers had directed attention to the circumstance that many cases of cardiac disease in their advanced stages,

and some even during every period of their course, may be unattended with any morbid murmur, the most frequent of such instances being, unquestionably, narrowing of the mitral orifice, in most cases of which, murmur, when present, is connected with a less advanced stage of contraction, and depends on imperfect closure of the valves, and therefore must generally be, as in the present instance, systolic in character.—*April 14, 1866.*

Rupture of the Biliary Duct.—Dr. M'SWINEY presented to the notice of the society an example of this very rare lesion. It was taken from the body of a man forty-five years of age, a bricklayer, who was brought to Jervis-street Hospital, on April the 14th, in a state of collapse and apparently moribund. Restoratives were employed, and after some hours reaction set in. Dr. M'Swiney saw him shortly afterwards, and received from him the following history of his case. He stated that he was awakened from sleep on that morning, at an early hour, by a very severe pain in his epigastric and right hypochondriac regions, with a feeling as if he wanted to evacuate the bowels. He went to the closet, but did not succeed in passing anything. He became gradually worse, with intense pain; and after the lapse of an hour or two vomiting set in; at noon he was taken to hospital; he further stated that he had recently suffered great privations; he was out of employment for some twelve or fourteen weeks, and declared that he endured what he called "downright starvation." He was suffering excruciating pain in the hepatic and hypogastric regions, and was vomiting a brown fluid, not stercoraceous, although it had the appearance as if feculent matter were mixed with it. The bowels were absolutely confined, and the secretion of urine suppressed; his pulse was very feeble, and almost imperceptible. He made several attempts to pass the contents of his bowels, feeling an inclination to go to stool, but ineffectually. In forty-eight hours after entering the hospital, and fifty-eight after the first attack, he died.

In making the *post mortem* examination, the abdominal cavity was the only one opened; the peritoneal sac was found to contain several quarts of fluid, which was stained dark green by bile. A very remarkable appearance was presented on the right side upon the parietal peritoneum, and also on the surface of the great omentum. The entire upper surface in contact with the liver, and in the neighbourhood of the liver, was coloured of the brightest yellow, as if it had been painted over with chrome yellow. The question at once arose, how the bile had become effused? He should have said that the gall bladder was perfectly empty and flaccid. There was no ulceration or rupture to be discovered in it; but a careful investigation soon enabled him to trace the common duct for the greater part of its length, and it was found to have been ruptured within about three-fourths of an inch of its entry into the duodenum, and just at its junction with the

pancreatic duct. About twelve inches from its termination the ileum became, quite suddenly, remarkably diminished in calibre and continued in this contracted and almost impervious condition down to its entrance into the cecum. Professor R. W. Smith had carefully examined the specimen, and was of opinion that the contraction had been the result of some morbid process of an obscure character; it was clearly not a congenital malformation of the tube; there was nothing to account for its occurrence, if it were not due to the intense spasms and pain which the man suffered during the acute attack of peritonitis. He believed that rupture of the common duct was a rare occurrence; and that when rupture took place, either in the gall bladder or in the ducts, it was generally due to the ulceration produced by a gall stone. These ulcerations were, for the most part, confined to the gall bladder; in the present instance there was no solution of continuity in the gall bladder, for it was inflated, as might be seen, for the purpose of showing its integrity; and there were no gall stones present in any part of the biliary apparatus, nor had the patient manifested during life any of the symptoms usually supposed to indicate the existence of calculi obstructing the ducts; there was no jaundice present during any period of his illness.

It was now a well-ascertained fact, that long privation of food usually caused an accumulation of bile in the gall bladder to an enormous extent. Taking this into consideration, along with the statement of the patient that he had suffered under prolonged deprivations of food, Dr. M'Swiney was induced to believe there was a violent and sudden rupture of the common duct, arising from over-distention, and that it occurred at the time when he was awakened from sleep (as he had detailed) by intense pain. That was, of course, but conjecture; but in the absence of any other reasonable explanation, he presumed to offer it as the solution of the difficulty. The omentum, it would be observed, was exceedingly fatty for a man suffering from starvation. He begged of his colleague, Dr. Hayden, to be good enough to look at a section of this yellow-coloured matter (so thickly pasted over the omentum, &c.) under the microscope, and to compare it with a section of some of the same substance which was not coloured. Dr. Hayden had written to him as follows:—"The layer of yellow matter in the great omentum consisted of fat deposited in granular masses, coloured deeply yellow with effused bile; in several of the fat cells thus coloured were seen crystals of margarine; the fat cells acted on by the bile seemed in most instances broken up, and in this respect contrasted with the uncoloured cells."—*April 21, 1866.*

Cirrhosis of the Liver.—DR. HAYDEN exhibited the thoracic and abdominal viscera, taken from the body of a female aged forty-two, who was admitted into the Mater Misericordiæ Hospital, October 3rd, 1865.

She stated that her health had been good till twelve months previously, when she coughed up a large quantity of dark blood; this she ascribed to the stoppage of the menstrual discharge, which occurred about that time; she had frequently since discharged blood, and on each occasion felt a peculiar, tinkling sensation in the left side of the chest; there had been no hemorrhage, however, for the six months preceding her admission. She had never been jaundiced, but about the period of the first occurrence of hemorrhage; her feet became swollen, and then her abdomen; he could not trace the abdominal enlargement from any particular point. At the date of her admittance into hospital her condition was the following:—She was very much emaciated; her face was of a dusky, pallid hue; the sclerotics white, tongue clean and moist, abdomen greatly enlarged, dull on percussion, tense and fluctuating; abdominal veins not enlarged; feet slightly œdematous; urine passed in moderate quantity, and free from albumen. There was cough with mucous expectoration; pulse quick and weak; respiration easy; the chest was universally resonant; respiration normal, but accompanied by mucous râles; heart's action and sounds normal.

After the patient had been a short time in hospital, it became necessary to perform paracentesis abdominis, to relieve her breathing, which had become greatly embarrassed; between five and six gallons of clear, amber-coloured fluid were removed. The abdomen so rapidly refilled, that the operation became necessary again three weeks subsequently, when a similar quantity of fluid, of the same character, was taken away. The patient had occasionally complained of pain in her left side; this she described as being of a shooting character, and extending from the left lumbar region upwards through the chest; the pain was more urgent after the last operation. About this time she coughed up a large quantity of puriform matter, which it was thought might have proceeded from an abscess of the liver, discharging itself through the lung.

About the 1st of December the left side of the chest became dull inferiorly, whilst the upper part yielded a somewhat tympanitic sound; the dulness gradually extended upwards; the heart was displaced towards the right side, and respiration became more embarrassed.

On December 12th the notes of her case show the following to have been her condition:—Decubitus on the left side only; pulse 104, and weak; respiration, 42; total loss of appetite and rest; left side of chest dull from second rib downwards in front; here the respiration was bronchial, and without vibration; above second rib, resonance highly tympanitic, and respiration and voice loud and ringing; posteriorly on the left side there was dulness, and absence of the respiratory sound, and of vocal fremitus below the angle of the scapula; above this level the respiration was tubular, and the dulness less marked; the heart pulsated to the right of the sternum.

On the 18th of December paracentesis was performed for the third time; about an equal quantity of a similar fluid was removed as on the previous occasions; the operation gave her comparatively little relief; pain in the left side and down the left arm was now most severe; there was, moreover, extreme tenderness over the left side of the chest anteriorly; this was most acute a little below and to the outside of the nipple, and beneath the clavicle; perspiration profuse.

December 22nd.—The left side exceeded the right in girth by two inches; physical phenomena unaltered, save as regards subclavicular resonance, which was less tympanitic; respiration and resonance were normal on the right side, or somewhat exaggerated. The spleen was felt firm and enlarged; no enlargement or other alteration of the liver could be detected by the touch; appetite good; pulse, 90; respiration, 30.

December 26th.—Muco-crepitus on the left side anteriorly; great fetor of the breath; slight enlargement of the superficial veins of the left side of the chest.

On the 27th of December the percussion note beneath the left clavicle was of the normal character, but slightly clearer than on the opposite side; and respiration was less loud, and accompanied with muco-crepitus in this situation; below the second rib dulness was complete, and no respiratory sound of any kind could be heard.

Early in February paracentesis was performed a fourth time; the fluid removed was similar in quantity and appearance to that previously described.

On the 27th tympanitic resonance was again present in the left subclavicular region, *i. e.*, to the level of the second rib, and on the following day dulness existed in this situation.

On the 2nd of March there was tympanitic resonance from the clavicle to the second rib, and as far as the middle line of the sternum horizontally; from the upper edge of the second to the upper edge of the third rib the resonance was less tympanitic, but below this level dulness was absolute.

The case was deemed one of considerable difficulty as regarded diagnosis, and no positive opinion was arrived at.

March 15th.—Paracentesis was performed for the fifth time; eight gallons of amber-coloured serum, containing much albumen, were drawn off; the patient, notwithstanding her great debility, bore the operation well. For several days preceding the operation resonance had been clear, but normal, in the left subclavicular region. On the 16th of March peritonitis of a subacute form set in, accompanied by vomiting, flatulent distention, &c., and acute tenderness of the abdomen.

March 19th.—The left side of the chest was now resonant throughout in front; the resonance was only exaggerated normal resonance; beneath the clavicle was felt a painfully sharp vocal thrill.

March 27th.—Copious night sweats set in; and, on April 9th, great

fetor of breath, and diarrhea. She died on the 18th of April; the physical signs on the left side of the chest having continued unaltered as on the 19th of March, and the heart having returned to the left side, and occupied a position a little above its usual level, as indicated by its pulsation to the inner side of the left nipple. The patient had been sinking for several days, being literally worn out by diarrhea and bed-sores which had latterly formed; shortly before death, she vomited up a large quantity of dark blood.

Autopsy.—Body much emaciated; a large quantity of greenish opaque fluid in the peritoneal cavity, with flakes of curdy lymph floating in it; the abdominal viscera were all firmly agglutinated to one another, and to the abdominal walls, by thick layers of false membrane; the parietal peritoneum was throughout coated with a similar layer, as thick as shoe-leather; the liver, which was firmly attached to the diaphragm by recently effused lymph, rather exceeded in volume the normal size of that organ, and afforded a good example of cirrhosis with fatty degeneration, being pale and granular on the surface, and in section devoid of vascularity, and thickly mottled with masses of yellow fat; examined microscopically with a magnifying power of 222 degrees, the hepatic substance was found to be devoid of blood; the hepatic cells were gorged with fat globules, which in most instances rendered the nuclei invisible; large oil globules were dispersed over the field; the colouring matter of bile was present in very minute quantity.

The spleen was of a dark slate-colour, and at least twenty times its ordinary volume; the mucous membrane of the large intestine was of a dark chocolate colour, and softened; the pelvic organs were all in a healthy condition.

The pericardium contained no fluid; the heart, which was of the ordinary size, presented a good deal of superficial fat. Both pleural cavities contained fluid; on the right side this was small in quantity and of a pale green colour, but clear; on the left side it nearly filled the pleura, and was of the colour of whey. The right lung was slightly emphysematous on its anterior margin, and hypostatically congested posteriorly, but otherwise in a healthy condition; the left lung was adherent to the anterior wall of the chest superiorly, and to the cone of the pleura; this portion (the superior lobe) of the organ was of the ordinary volume, and resonant on percussion; but the inferior lobe was compressed into the posterior inferior portion of the pleural cavity, where it was bound down, and generally coated over, by a thick layer of false membrane; it was much reduced in volume, solid, and dull on percussion.

The tympanitic resonance occasionally present beneath the left clavicle during the patient's illness, as previously noted, and which led to so much speculation in regard to its cause, was manifestly due (for there

was no other cause that could have given rise to it) to the presence in this situation of the superior lobe of the lung, where it was confined by adhesion, and subjected to the varying pressure, according to the quantity, of the liquid which occupied the lower portion of the pleural cavity, as well as of the peritoneal effusion exercised through the diaphragm. When the liquid in the pleura increased, or the diaphragm was pressed upwards by the ascites, stasis of air in the superior lobe was the consequence, which then yielded, through that portion of the chest wall to which it was attached, a tympanitic sound; but when it was allowed to expand by removal of pressure from below, whether by the operation of paracentesis, or by more active excretion through the kidneys, &c., it yielded a resonance and respiratory sound, exaggerated only because of its close attachment to the thoracic wall.

The liver was the seat of the primary disease in this case; the spleen became hyperemic and hypertrophied in consequence of the obstruction in the liver, as noticed by Professor Smith many years since; and owing to retarded or obstructed portal circulation in both these organs, ascites was superadded, and hematemesis occurred. In the further progress of the case effusion took place into the pleuræ; and on the left side, owing to the pre-existence of old pleuritic adhesion of a somewhat peculiar kind, the pleural effusion gave rise to physical phenomena of a novel and very embarrassing description.—*April 21, 1866.*

TRANSACTIONS OF THE MEDICAL SOCIETY OF THE COLLEGE OF PHYSICIANS.

DR. BEATTY, President.

DR. MADDEN'S *Paper on Insanity.*

(Continued from page 230.)

However, although insanity is too often punished as crime, on the other hand, crime sometimes shelters itself under the disguise of insanity. For my part, I am not one of those who share Lord Hale's opinion, that "all crime is the result of partial insanity:" a dogma which appears to me not only subversive of the principles of all religion, and dangerous to society, but at variance with common sense.

Mere passion is not madness. Nor should any, so called, irresistible impulse, not connected with a diseased brain; nor any emotion or custom which is not of itself a proof of insanity, be considered as conferring immunity from the just punishment of crime. None are free from passions or impulses, which if they be not checked may become almost irresistible from habit, and may lead to crime. But in such cases the perpetrators of crimes being accountable for the acts by which the control

over the passions was originally weakened, they are equally accountable for all the consequences that may arise therefrom. A madman is not thus responsible, not being answerable for the diseased action in his brain whence the insane act proceeds.

The law of England, as laid down by the judges in their reply to the queries of the House of Lords on this subject, is—that if the perpetrator of an action is capable of distinguishing right from wrong at the time he committed it, he is legally responsible for it, even though he may be partially insane. The following are the words of this decision:—

1st. “Notwithstanding that the party committing a wrong act, when labouring under the idea of redressing a supposed grievance, or injury, or under the impression of obtaining some public or private benefit, he was liable to punishment.

2nd. “That before a plea of insanity should be allowed, undoubted evidence ought to be adduced that the accused was of diseased mind, and that at the time he committed the act he was not conscious of right or wrong.” “Every person was supposed to know what the law was, and therefore nothing could justify a wrong act, except it was clearly proved the party did not know right from wrong. If that was not satisfactorily proved the accused was liable to punishment.”

The 3rd question was not answered, and as it was purely legal need not be quoted.

4th. “The judges were unanimous in opinion, that if the delusion was only partial, that the party accused was equally liable with a person of sane mind. If the accused killed another in self defence he would be entitled to an acquittal; but if committed for any supposed injury he would then be liable to the punishment awarded by the laws for his crime.”^a

The 5th and last question refers to the examination of experts as to the patient's state of mind at the time when he committed the crime, and, with the exception of Justice Maule, it was decided that the question was one for the jury, and should not be put to the witness except under certain special conditions.

Similar views have been at all times promulgated from the judicial bench in England. On the trial of Arnold, in 1723, for shooting at Lord Onslow, Mr. Justice Tracy stated that a lunatic to be exempted from punishment for his acts “must be a man that is totally deprived of his understanding and memory, and doth not know what he is doing no more than an infant, than a brute, or a wild beast.”^b In 1812 Mr. Justice Le Blanc, in a case of murder tried before him at the Old Baily, instructed the jury thus:—“Provided you should be of opinion that when

^a Hansard's Parliamentary Debates, 1843.

^b Hargrave's State Trials, 322; and Collinson on Lunacy, Vol i., p. 672.

he (the prisoner) committed the offence he was capable of distinguishing right from wrong, and was not under the influence of such a delusion as disabled him from distinguishing a wrong act, in that case he was answerable to the justice of his country, and guilty in the eye of the law." Chief Justice Mansfield, in his charge in the case of Bellingham, who was tried for the murder of Mr. Percival, told the jury that if the prisoner fancied the existence of an imaginary injury, and sought redress for this by some illegal act, but "was capable in other respects of distinguishing right from wrong, there was no excuse for any act of atrocity he might commit under this description of insanity;" and Lord Lyndhurst, on the trial of Offord in 1831, expressed his coincidence with Chief Justice Mansfield's charge; and, further, informed the jury that they would not be justified in acquitting the prisoner on the ground of insanity unless they could answer this question in the negative, viz. :—"Did the prisoner know that in committing the act he offended against the laws of God and man?"^a Sir William Follet, then Solicitor-General, who prosecuted M'Naughten for the murder of Mr. Drummond in 1843, stated the law to be that "to excuse him (the prisoner) it will not be sufficient that he laboured at the time under partial insanity, that he had a morbid disposition of mind which would not exist in a sane person; that is not enough; if he had that degree of intellect to enable him to know and to distinguish right from wrong, if he knew what would be the effect of his crime, and if with that consciousness he wilfully committed it."^b

On the memorable trial of Townley, convicted of the murder of Miss Goodwin in 1863, Baron Martin addressing the jury said:—"In his opinion the law was best laid down by Justice Le Blanc, as able a judge as ever sat on the bench. Justice Le Blanc, in the case alluded to, observed to the jury that it was for them to determine whether the prisoner, when he committed the offence with which he stood charged, was incapable of distinguishing right from wrong, or under the influence of any illusion which rendered his mind insensible of the nature of the act he was about to commit. Since in that case he would not be legally responsible for his conduct. On the other hand, provided they should be of opinion that when he committed the offence he was capable of distinguishing right from wrong, and not under the influence of such an illusion as disabled him from discerning that he was doing a wrong act, he would be amenable to the justice of his country, and guilty in the eye of the law."^c

This "knowledge of right and wrong" which is made the legal proof of criminal responsibility is, perhaps, as fallacious a test of mental

^a Corrinton and Payne's Reports, Vol. v., p. 168.

^b Annual Register, 1843—Law Cases, p. 347.

^c Annual Register, 1863—Law Cases, p. 308.

sanity as could well be devised. The majority of the inmates of our lunatic asylums have, to a certain extent, the power of distinguishing between right and wrong. For they often adopt precautions and practice concealment when doing acts they know to be wrong, thus clearly showing that they are aware of the impropriety of their conduct, and are anxious to escape its punishment. And yet, such patients may, in other respects, be obviously insane.

The expounders of our law appear to me in this point to confound conscience with consciousness. How is this abstract "knowledge of right and wrong" to be decided? Is it by the conscience of the accused? or by public opinion which differs so widely in different times and places? or by the civil law, which in one state makes that a crime which in an adjoining country may be none? Is the question to be tried by the divine law, acting in self-supposed conformity to which men, otherwise of sane mind, have conscientiously perpetrated actions which the great bulk of mankind regard as reprehensible. Surely, therefore, the so-called knowledge of right and wrong cannot, of itself, be regarded as a sufficient test of sanity and consequent responsibility.

It would be easy to quote numerous cases in illustration of the consequences of the inhumane law that has reigned and still is enforced in England, with reference to criminal lunacy. I shall, however, content myself with alluding to some of the most notorious cases in which either the sentence of death has been pronounced, or has actually been carried into execution on persons in the state of madness.

It will hardly be necessary before giving these cases to prove that it can be neither lawful nor expedient to inflict punishment on a lunatic. Although I might quote curious passages from Hansard's Parliamentary Debates (Volume 67) to show that this practice has been vindicated by a Right Rev. Prelate who undertook the Christian office of proving that dangerous lunatics should be dealt with after the fashion of mad dogs, and should be destroyed for the good of society. But as this humane sentiment is not often so explicitly avowed, I shall merely quote the words of two eminent lawyers:—

"The only way to look at the punishment of death," says Lord Wharncliffe in his evidence, "is to say—is it an example or not? If an example, it is defensible; if it is not, it is not defensible."^a There can be no question that a madman, not being under the dominion of reason, cannot be acted upon by the force of example, and thus deterred from an insane act. And, therefore, any punishment is unjust; for, as Mr. Stephens says—"The end or final cause of human justice is not, by way of atonement or expiation for the crime committed—for that must be

^a Evidence of Lord Wharncliffe—Commissioners Report, 1836, p. 96.

left to the just determination of the Supreme Being—but as a precaution against future offences of the same kind.”^a

It seems to have been generally admitted that Bellingham, who was tried for the murder of Mr. Percival, was improperly executed, the application of his counsel for a short postponement having been refused, although witnesses could have been then produced to prove clearly his insanity. The case of Bowler affords another example of the execution of a madman subject to epileptic fits, and exhibiting all the symptoms of mania. Baron Alderson afterwards, at the trial of Oxford, justly remarked, with reference to the last case:—“Bowler was executed, I believe, and very barbarous it was.” In the case to which I have already referred, which was tried before Mr. Justice Le Blanc, at the Old Baily, 1812, a prisoner was charged with shooting at a man named Burrowes, and although a commission of lunacy had found him insane only one month before he committed the offence, still the jury brought in a verdict of guilty, and he was accordingly condemned and executed.

John Barclay who was executed at Glasgow in 1833, was also of unsound mind. He was familiarly known as “Daft Jack.” The clergyman of the parish had always regarded him as imbecile. He thought that a watch was a living animal; and that there was no distinction between killing an ox and killing a man. Yet, although Barclay’s weakness of mind was recognised both by the judge and by the jury, who, on that ground recommended him to mercy, still he was condemned and executed on the plea, forsooth, that “he knew right from wrong.”

John Howison, who was tried before the High Court of Justiciary in Scotland, in December, 1831, and convicted of the murder of the widow Geddes, and subsequently executed—was undoubtedly insane. He was a peddler, and up to a certain period was a man of orderly habits and ordinary manners; immediately after a fever his habits changed completely. He became morose, silent, fond of solitude, and superstitious, wearing a Bible tied to his head, or dangling from his wrist. He sprinkled salt on his bed, and used to sit for hours brushing away imaginary flies. His appetite became enormous, and he contracted a craving for blood, and would eat an incredible quantity of bullock’s liver nearly raw, and slept always with weapons under his head. At one time he became a Quaker, but during the worship would sit muttering to himself, and running pins and needles into his wrists and arms, would suck the blood with great gusto. Moreover, he complained of pain and uneasiness in the head. The crime for which he suffered was apparently quite motiveless; and he accused himself of eight other murders, none of which had ever occurred.^b

^a Stephens—*New Commentaries on the Laws of England*, Vol. iv., p. 64.

^b *Edinburgh Medical and Surgical Journal*, Vol. xxxviii., p. 51.

In 1862 Charles Fokes was tried for the murder of Daniel Stone at Walditch on the 29th of August. Immediately after the murder he had attempted to shoot himself. There appeared to be no motive for the act, except that he had some vague notion that people, and his victim among the number, laughed and jeered at him. Dr. Tuke, who examined the prisoner, said:—"My examination left no doubt on my mind but that he is at this moment of unsound mind. . . . I do not believe him responsible for his actions." The judge's charge in this case was the strongest proof of the incompetency of an unprofessional tribunal to deal with cases of lunacy. He told the jury:—"Although you may be satisfied that this man had some insane delusions; yet if at the time he did this act he knew the nature of that act, and what the consequence would be, and also that it was wrong, then it is your duty to find him guilty." This criminal lunatic was accordingly found guilty, and sentenced to be hanged.

At the Maidstone Assizes, at the same time (1862) a lad aged eighteen, named Burton, was tried for the murder of a boy, between nine and ten years old, on the Chatham Railroad. The prisoner had given himself up voluntarily. There was no cognizable sane motive whatever for the crime. The prisoner having expressed an insane desire of being hanged, had apparently committed the murder solely to accomplish that object. Burton's mother had been twice in a lunatic asylum; his brother was foolish; and he himself was generally considered of weak mind. Medical evidence was given which sustained this opinion. But Mr. Justice Wightman, in summing up, is reported to have said, that the prisoner "was supposed to desire to be hanged, and in order to attain that object committed murder. That might show a morbid state of mind but not delusion. Homicidal mania, again, showed no delusion." So, accordingly, Burton was found guilty, and sentenced to be hanged.

George Victor Townley, who was sentenced to death at the Derbyshire Winter Assizes in 1863, for the murder of Miss Goodwin, a young lady to whom he had been engaged to be married, was described as being "a man of very quiet and refined manners, a good linguist, and an accomplished musician." The defence set up was insanity. Some members of the prisoner's family were proved to have been mad. Dr. Winslow in his evidence said:—"His (the prisoner's) moral sense was more vitiated than I ever saw that of any other human being. . . . I think that at this present moment he is a man of deranged intellect." Similar evidence was given by others; and his conduct after the murder, when he gave himself up to justice, assisted to carry home the body of Miss Goodwin, and then took tea with her grandfather, was also ably set forward by his counsel as proof of insanity. The jury, however, took only five minutes to consider the case, and returned a verdict of guilty, and he was sentenced to death. The presiding judge, Baron Martin, next day

made a communication to the Home Secretary, the result of which was that forty-eight hours before the time fixed for his execution he was examined by the Commissioners in Lunacy, who reported, "that (for certain reasons given) the prisoner could *not* be considered as being then of sound mind; but that according to the law, as laid down by Baron Martin, which was in accordance with the highest legal authorities on the subject, he was responsible for his actions!!!" Most fortunately the same post brought a certificate, signed by three magistrates and two medical men, in accordance with the 3rd & 4th Victoria, cap. 54, testifying to the insanity of Townley, who was respited on this report.

The subsequent proceedings in this case are not a little curious. Thirty-nine visiting justices of Derbyshire, with an enthusiasm worthy of Dennis, the hangman in *Barnaby Rudge*, signed a strong protest against the respite. Public meetings were held, at which, and in a large section of the press, a torrent of invective was showered upon Sir George Grey, on those who had signed the certificate of lunacy, and on those who had given evidence of Townley's insanity. The effect of all this was, that a second commission were appointed to again report on the prisoner's mental condition. These gentlemen came to the conclusion that Townley was of sound mind, and he was again in danger of being executed. Ultimately the sentence was commuted to penal servitude for life, and, finally, this wretched lunatic, by committing suicide in prison, justified the opinion of those who pronounced him mad.

In the Official Report on Irish Prisons for 1864, p. 8, I find the following case, ably reported by Mr. Lentaigne, the Inspector-General of prisons, which, although less certainly a case of insanity than those I have already alluded to, is one, at any rate, very closely resembling some of those very remarkable cases mentioned by Dr. Winslow, and other psychologists, in which murder has been connected with the influence of some impression made on the mind in sleep by a dream. Dreaming, I may here remark, being a state that presents many analogies with insanity.

Mr Lentaigne says:—"In April, 1864, an execution took place in front of this (the Cavan) gaol, for a murder committed on the 23rd January, under circumstances well deserving attention, as connected with penal legislation. Bernard Cangle lived, fourteen years ago, as farm-servant with Peter Reilly, a farmer, in comfortable circumstances. During that period an ass, belonging to Reilly, bit the left hand of Cangle so severely that it had to be amputated. On his recovery he demanded compensation, which Reilly refused to give. Soon after, as I understand, Cangle went to America, but returning, was convicted of cow-stealing at Trim, and sentenced, on the 9th July, 1861, to three years' penal servitude. During the entire period of his detention in the convict depôt he never committed a single breach of prison rules; and

on one occasion, when a fellow prisoner was guilty of an offence, he gave information to his warder. On the 9th January, 1864, he was liberated on license, and, after remaining a few days in Dublin, went to Reilly's house in Cavan, where, on telling his name, he was treated with the greatest hospitality and kindness, and asked to sleep on the loft with the servant boy. Between two and three o'clock in the morning he came down from the loft, only half dressed, saying, "he could not sleep as there were lights or flashes of fire outside the house." Reilly got up to see what was the matter, and was immediately stabbed to death by Cangle, who also stabbed Reilly's wife when she came to her husband's assistance. He then fled, having only his shirt and trousers on, and at once gave himself up at a police barrack some miles distant, saying, "he had stabbed Reilly." The greatest pains were taken by the convict authorities to ascertain if, during the period of his imprisonment, he had ever shown the slightest indication of insanity, but all the medical officers of the convict depôts agreed he was perfectly sane, and the prison officials in Cavan are of the same opinion. It is, therefore, evident that either he had nursed his revenge for the injury during nearly fourteen years, or else on the night in question some sudden impulse had roused him to commit the crime, even at the sacrifice of his own life; but this is negatived by the fact of his having the knife in his possession. The case is one of those psychological phenomena, the facts of which should be truly recorded, especially at a time when acquittals, on the grounds of insanity, are so easily obtained."

The Inspectors-General, however, evidently feel themselves in some degree restrained by public opinion, but they observe, in the above quoted report, that acquittals in similar cases are not sufficiently easy (query possible) to be obtained.

On the 20th of December, 1865, Stephen Forward, alias Southey, who was charged, in August last, with the murder of three boys in London, and also of the murder of his wife and child a day or two after at Ramsgate, was arraigned, tried, and found guilty, before Mr. Justice Mellor, at Maidstone, on the latter charge. The particulars of this case are too familiar to all of us to need any *precis* of the facts. And the execution of this wretched man appears to me, in itself, a terrible commentary on the injustice of our laws relating to the investigation and punishment of criminal lunacy.

The last of these cases which I shall refer to is that of Andrew Brown, who was tried before the High Court of Justiciary at Edinburgh, last assizes, and executed at Montrose in January, 1866. The prisoner was the mate of the schooner *Nymph*, and was tried for the murder of the captain of that vessel, at sea, off the Forfarshire coast.

On his trial the prisoner pleaded generally "not guilty," and pleaded specially that at the time the act was committed he was labouring under temporary insanity.

The following is the substance of the evidence :—

“John Pert; sailor, deponed,—I was engaged, on the 4th of September, to go a voyage on board the steamer *Nymph*, of Montrose. There were on board, besides myself, Alexander Reaburnes, Andrew Brown (the prisoner), and John Greig (the master). The prisoner was mate. I first saw the prisoner two or three days before we sailed. We were all sober when we sailed. The prisoner was so, so far as I saw. I had seen him every day for two or three days before that, and had not observed him to be the worse of drink. The master was asleep and snoring when he was killed. Brown slipped forward and went to the fore-castle. I did not see him come aft, but I found afterwards that he had slipped behind me. My attention was first directed to that by hearing two heavy blows. I looked over my right shoulder on hearing them, and saw the master's head lying in two halves. I was stunned for a little bit; and Brown, who had the axe up, let it down again before I could get hold of him. The two blows I first heard were given quickly one after the other, and the prisoner had the third blow struck before I had time to interfere. I then rushed into the bulwarks and took the axe out of his hand and threw it overboard. The master was apparently dead by this time. After I had thrown away the axe Brown said :—‘I have done the deed, and I will have to suffer for it.’ *A few minutes afterwards he asked me if I would come and see him hanged. About five minutes after I had seized the axe from him he said, ‘Jack, it is a good job you got the axe, or else you would have got the same.’* I had a good twist with him before I could get the axe out of his hands. When I was struggling with him he did not speak. I am sure he was sober at that time. I do not think he had much drink that day. Before we went to dinner on shore we had had a drink of ale together, but that was all. The prisoner then took the helm, and steered the vessel for Stonehaven. He spoke about his mother, and in course of conversation, said, but that he wished to see her he would go over the side. I understood by that he would drown himself, but that he wanted to see his mother. He asked the loan of a shilling from me after we passed Bervie, and said he had a sixpence, and he would give one shilling and sixpence to his mother. He said it would be the last money he would ever give her. He asked Reaburnes and me to wash up the blood, which we did. After the disturbance that had taken place on board we were afraid of Brown. He asked me if I would come to see him hanged. I made no answer. He was not weeping at any time. He said, ‘*Jack, I am going stark mad—out of my mind.*’ This was said about two hours after the deed, and while he was steering.”

Alexander Reaburnes corroborated the evidence. He said :—“Prisoner told us, ‘*I'm master of the ship, and want to go to Stonehaven to see my mother.*’ We tried to get from him, the best way we could, the reason for the murder, and he said, with great violence, and slapping his breast, ‘*I have*

another to kill.' We asked what he had done this for. He simply replied, in a lamenting way, 'I have done the deed, and will have to suffer for it.' We asked him who the other person was he had to kill, but he would not tell us. I do not think he meant himself. I did not observe any difference in his spirits after the murder. He was not like a drunken man. He was not swearing or using any harsh language during the afternoon. In sailing into Stonehaven he always gave the orders, and gave them correctly. If this deed itself had not been done, there would have been nothing about him to attract attention at all."

Cross-examined.—"I saw no exhibition of ill-feeling between the master and the prisoner."

"Evidence was then called for the defence, to show that the prisoner had sustained a fall in boyhood, and that he had had a fall four years ago which led to a surgical operation; that a block had fallen on his head about a year ago, and that he had suffered other bodily injuries, which had, his sisters deponed, changed his disposition from cheerful to sullen, and that he had gradually fallen into habits of drinking. The witnesses stated that very little drink produced great excitement in him, and some thought he was not right in his mind. As one witness expressed it, 'The dram went to his brain.' "

A verdict of guilty, however, was brought in, in face of this testimony, and he was sentenced to be executed on the 31st of January.

The foregoing cases in which sentence of death was pronounced or carried into execution on madmen might be very easily multiplied, but I think I have brought forward quite enough to prove that, by the laws still enforced in England, human life has often been judicially sacrificed, and death has been made the punishment of the most dire calamity that can befall man; an affliction, too, which may visit the best and wisest of mankind, and against which no precaution can be taken, and no means of prevention adopted—the loss of reason.

We are often told that the inhumane and barbarous customs of our ancestors would not now be tolerated by the advanced civilization and enlarged philanthropy of the nineteenth century. But I greatly fear that any one who considers the subject which this paper treats of will find too much evidence to prove that we overrate the moral effect of the superior civilization of the present day, and its influence in restraining the evil passions of human nature, its cruelty and desire for vengeance, and not for justice. It is true that the insane are no longer lashed and chained in madhouses as they were formerly; but it is not less true that they still may be, and often are, hung by the neck until they are dead, in punishment for acts committed under the impulse of madness.

I trust that, by the foregoing observations, I have succeeded in showing how uncertain and unjust is our present law in reference to the criminal responsibility of the insane; but a recent official document, the *Report of*

the Capital Punishment Commission, 1865, proves far more cogently than I could do so, the necessity of an amendment of the laws relating to, and the mode of determining, the mental capacity of criminals charged with capital offences. In the 18th section of that report the Commissioners say:—

“18. There are other questions of great importance upon which we have taken evidence, viz.:—(1.) The propriety of allowing an appeal on matters of fact to a court of law in criminal cases. (2.) The mode in which the crown is advised to exercise the prerogative of mercy by the Home Secretary. (3.) The present state of the law as to the nature and degree of insanity, which is held to relieve the accused from penal responsibility in criminal cases. It is obvious that these difficult questions are not confined to capital crimes only, but pervade the whole administration of criminal law. They therefore require a more general and comprehensive treatment than the terms of the commission under which we act will admit. We think, therefore, that while we should not be justified in making any recommendation to your Majesty on any of these points, we should fail in our duty did we not humbly solicit your Majesty's attention to them as requiring further investigation.”

It, however, can surely need little evidence to prove that laws which are connected and deal with a scientific question, and which were enacted when that branch of science was in its infancy, and are in a great measure founded upon the false theories and imperfect knowledge of the time when they were made, are now far behind the present state of psychological information, and therefore require revision. To amend the present state of the law of criminal insanity I would propose two very short and simple, yet, I believe, most effectual, measures, viz.:—

1st. The 64th article of the French code should be adopted by our law in its entirety.

2nd. The question of madness should be decided by a tribunal of experts.

The article of the Code Napoleon to which I refer is as follows:—

“Il ny a ni crime ni délit, lorsque le prévenu était en état de démence au temps de l'action.”^a

In the State of New York a similar law has been long enacted:—

“No act,” says this statute, “done by a person in a state of insanity can be punished as an offence, and no insane person can be tried or sentenced to any punishment for any crime committed in that state.”^b

In cases in which the defence of insanity is set up, the English law, as laid down by Mr. Justice Mellor, on the trial of Southey at Maidstone, December, 1865, is that “the question of the fitness of the prisoner to be

^a French Penal Code, Article 64.

^b Revised Statutes, vol. ii., p. 697.

tried should be determined before he was called on to plead. But in a case which occurred before his brother Blackburn, where it arose in the course of the trial, he put both questions at the end of the trial—whether the prisoner was in a fit state to be tried, and whether he was guilty or not guilty. There would be a great loss of time and great inconvenience in having the question tried twice over.”^a

By the 39th of George III., cap. 44, it is provided that—“If on the trial of any person for murder or felony, he appear to the jury charged on the indictment to be insane, the Court may order the finding to be recorded, and that he be kept in custody till Her Majesty’s pleasure be known.” It was moreover enacted by the 39th and 40th George III., cap. 94, and also by the 3rd and 4th of Victoria, cap. 54, that, even after sentence has been pronounced, “it shall be lawful for any two justices of the peace of the county or place where any person under sentence of death is confined, to inquire, with the aid of two physicians or surgeons, as to the insanity of such person; and, if it shall be certified by such physicians and surgeons that such person is insane, it shall be lawful for one of Her Majesty’s principal Secretaries of State to direct, by warrant, that such person be removed to a proper receptacle for insane persons.” This, however, was merely a permissive law, and was so seldom acted upon that when it was put in practice in the case of Townley, the press and public opinion were vehement, and almost unanimous in their condemnation of those who had ventured to save thus a madman from the gallows. By the 27th and 28th of Victoria, cap. 29, an important modification was made in the law, by which the power of medical inquiry into the state of mind of persons under sentence of death was confined to physicians and surgeons specially appointed for this purpose by one of her Majesty’s Principal Secretaries of State.

The second point on which I would insist, namely, that the question of insanity should be decided by experts, is unhappily opposed by the weight of public and legal, as distinguished from humane and scientific, opinion.

There is, unfortunately, a great aversion to the reception of medical evidence in cases of insanity, and such testimony carries little weight with either the judges or juries in most cases.

The opinion tersely expressed by Lord Chancellor Westbury, in the House of Lords, not very long since, when he spoke of “the vicious principle of considering insanity as a disease,” seems to prevail widely in this country. The Earl of Shaftesbury, in his evidence before the select Committee of the House of Commons on lunatics, went so far as to say:—“For my own part I do not hesitate to say, from very long experience, putting aside all its complications from bodily disorder, the mere judgment

^a The Times, December 21st, 1865.

of the fact whether a man is in a state of unsound mind, and incapable of managing his own affairs, requires no professional knowledge. My firm belief is that a sensible layman conversant with the world and with mankind can give not only as good an opinion, but a better opinion than all the medical men put together. I am fully convinced of it." At the trial of Foakes for murder, August 21st, 1862, the learned judge thus instructed the jury:—"If common sense is not to decide you ought not to be in that box; as juries do not pretend, and are not expected to have any scientific knowledge. . . . You are not to be deprived (added the judge to the jury, proudly swelling with the sense of their own sagacity and importance) of the exercise of your common sense, because a gentleman comes from London and tells you scientific sense." Accordingly the jury exercised this common sense, and showed their proper contempt for science by finding a man stark mad, guilty of murder.

It would, of course, be misplaced and unnecessary here to stop for the purpose of discussing the necessity of scientific evidence, and the fallaciousness of those views which lead to its rejection. That insanity is a disease is patent enough to medical men who are well acquainted with the intimate connexion of mind and body; the relation which the health of the latter bears to that of the former, and the influence exercised by medicine over the diseased conditions of both; but it would take more time than I have now at my disposal to prove this proposition to lawyers and jurymen equally ignorant of physiology and psychology.

The difficulty of recognizing insanity, and of distinguishing between real and feigned insanity, and the consequent danger that under the plea of unsoundness of mind, madness may be feigned, and thus lead to the immunity of criminals, is probably the explanation of the reluctance with which judges and juries generally entertain that plea. This very difficulty is, however, the most powerful argument against the present system of dealing with such cases.

Nothing can probably require more discrimination, care, and tact than the recognition of some of the less evident forms of mental disease, or than to distinguish between real and simulated insanity. Such knowledge of the human mind cannot be acquired without special training and experience. And even with such training it is often a matter of extreme difficulty to discover evidence of insanity when delusions the most strange and strongly impressed on the mind actually exist. I was recently requested to visit a gentleman in a lunatic asylum near Dublin, for the purpose of signing a certificate. Yet such was the insane cunning with which he managed to conceal his madness, that although I was previously aware of the nature of his delusion, and conversed with him on the point for an hour, and though he presented the peculiar aspect of insanity which is more familiar to the attendants on the insane, than easy to describe, still I felt obliged to leave without signing the certificate. I, however,

subsequently sought an opportunity of again seeing this man, and then, at a single interview, ascertaining his madness, signed the paper. This I mention only as one of the instances which almost every practitioner meets with, showing the difficulty of pronouncing a person sane or mad.

No rational man would be likely to take the opinion of a jury of grocers, publicans, or tallow chandlers, as to whether his lungs were sound, or his watch in order, in preference to those of a physician or a watchmaker. And yet a question far nicer than either of those, the question of a man's healthy or diseased psychological condition—of all scientific investigations the most delicate and difficult—is, by the laws of England, left to the decision of such persons, assisted by a judge whose knowledge, however extensive it may be in legal matters, is hardly, if at all greater than theirs on any department of medical science.

But besides the foregoing, there is, I think, another cause assignable for the unfavourable opinions expressed by lawyers and judges as to the value of medical evidence in cases of insanity. They, perhaps, may consider that medical men, when employed either by the government as witnesses in cases of alleged criminal lunacy, or by either party to a private case of imputed insanity, may, and very likely unconsciously, be influenced by the fact of being produced or preferred by one side or the other—and thus sometimes, it may be, have the opinions they express more or less affected by the views of their employers.

On such occasions, and when placed in such a position, medical men should, however, bear in mind what they are, and consider themselves as members of a pre-eminently exalted, ennobling, and humanizing profession, having the sacred offices of a high and holy calling to perform, and bound at all hazards, and at every sacrifice of self-interest, to sustain the ends of truth and justice alone.

As lawyers predominate in the legislature, we can hardly hope to wrest the question of lunacy—although it is as medical a question as fever or consumption—from their control, but still something might be effected towards reforming and obviating the recurrence of the abuses I have indicated. If the voice of the profession were unanimous in urging the great importance of the subject on public attention, it might be possible to obtain the appointment of medical assessors in lunacy, who, as the nautical assessors do in marine cases, should advise the court on all professional points. This would to a great extent do away with the necessity for the examination of numerous witnesses in lunacy cases, save the public time, and would avoid the perplexing contradictions of conflicting professional opinions.

There are many other topics connected with the subject of insanity and criminal responsibility which my space prevents my even alluding to. I shall therefore conclude with expressing an earnest hope that a wiser and more humane spirit may before long pervade the laws of this country

relating to so-called criminal insanity; and that the most terrible punishment that man can inflict shall no longer be awarded to the unfortunate victims of disease.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.*

TWENTY-EIGHTH ANNUAL SESSION.

DR. M'CLINTOCK, President.

Rupture of the Vagina during Labour.—DR. BYRNE read a case in which the patient recovered after this accident.—10th March, 1866.

Rupture of the Uterus.—DR. ISDELL read a case of rupture of the uterus in which the patient recovered.—12th May, 1866.

Placenta Previa.—DR. ISDELL read a case of placenta previa, with remarks on the treatment of this complication.—12th May, 1866.

Pregnancy Complicated with Sub-acute Peritonitis and Ascites.—DR. RINGLAND detailed a case of this complication.—12th May, 1866.

Double Monstrosity.—DR. M'CLINTOCK read a paper on this subject (See page 192.)

Medicated Pessaries.—DR. KIDD exhibited a new form of medicated pessary, made for him by Mr. Pakenham, of Henry-street, since he had last brought the subject before the society. Dr. Kidd begged to remind the members that, on the 13th January last, he exhibited some pessaries made in Edinburgh, for Sir James Simpson's use, and some of the same description made by Mr. Pakenham. These were composed of cocoa-nut butter, a substance sufficiently firm at low temperatures to allow of the pessary being introduced into the vagina by the patient herself, but which melted very rapidly at the temperature of her body; but he then said that in practice he had found these pessaries were often objected to by patients, because, as the cocoa-nut butter melted it escaped from the vagina, and soiled the clothes in a very offensive manner, and that, to obviate this difficulty, he had latterly got the pessaries made as small as suppositories, so that, instead of containing 120 or 150 grains of

* These Reports are furnished by Dr. Geo. H. Kidd, Secretary to the Society.

cocoa-nut butter, they contained only five or ten. But a pessary of this size would not contain a sufficient dose of many of the medicinal agents that may be applied to the uterus with advantage, and it is difficult to properly introduce such small pessaries into the vagina; so that it becomes necessary for the medical man to apply them himself, by means of the speculum. This latter objection, Dr. Kidd said, applied also to another means of applying medicinal substances directly to the uterus, viz., that suggested by Dr. Tilt, by wrapping them up in cotton, and introducing it into the vagina, as this can only be done rightly by means of the speculum.

The requisites, Dr. Kidd said, for a good form of medicated pessary are:—

1st. That it can be introduced by the patient herself.

2nd. That it will bring the medicinal agent into contact with the mucous membrane of the vagina and uterus, and retain it there sufficiently long to allow of absorption, or of such local action as may be required.

3rd. That it will not be offensive to the patient, soil her clothes, or prevent the medicine having due effect, by allowing it to escape from the vagina.

The pessary now exhibited by Dr. Kidd complied with all these requisites, and had none of the disadvantages of the other forms, nor did it require the use of an instrument for its introduction into the vagina, like that recently suggested by Professor Raccioborski. It was composed of cotton, coated with an exceedingly thin layer of cocoa-nut butter, so thin and so small in quantity as to be unobjectionable. In manufacturing this pessary, Mr. Pakenham rolls a small portion of cotton, to which a thread has been attached, on the end of a glass rod, giving it the form and size of the ordinary conical medicated pessary. He now dips it rapidly into melted cocoa-nut butter, so as to give it a uniform thin coat, to preserve its shape, and give it firmness. As soon as the cocoa-nut butter is cool, the glass rod is withdrawn, and the pessary now resembles an empty cartridge case, which may be charged for use by introducing into it the medicinal agent to be employed, and which may be used either in the state of dry powder, or mixed with glycerine or water. The end of the charged cartridge is now plugged with cotton and cocoa-nut butter, and it is ready for use.

It has now a form and consistence to allow of its easy introduction into the vagina by the patient herself. The layer of cocoa-nut butter soon melts, and mingles with the cotton, serving to bring the drug, with which the cartridge is charged, into contact with the vaginal mucous membrane, and allow of its action on it. When the pessary is introduced, the thread, which had been attached to the cotton, is allowed to hang out of the vagina, and serves as an easy means of withdrawing the cotton, when a sufficient time had elapsed for the action of the medicine.

Dr. Kidd stated that he had used these pessaries, charged with various substances, such as tannin, alum, iodides and bromides of potassium, iodine dissolved in glycerine, acetate of lead, opium, morphia, &c., and had found them most satisfactory; but he had learned that active preparations, such as tannin, which may irritate the mucous membrane, or morphia, should be used in smaller doses than in the old form of pessaries.—9th June, 1866.

Large Fibrous Interstitial Tumour of Uterus.—DR. JOHN A. BYRNE exhibited to the society a very large fibrous tumour of the uterus, and stated that he regretted being unable to furnish any clinical history of the case to the members.

He was indebted for it to his friend Dr. Cruise, who, in making a *post mortem* examination of a patient under his care in the Mater Misericordiæ Hospital, who had died with apoplectic symptoms, removed it. Dr. Cruise was aware of the existence of it before the death of the woman, having felt the mass over the pubes when he was introducing the catheter to empty the bladder of the patient; this was rendered necessary by the state of coma in which she lay, and not by any obstacle caused by the pressure of the tumour itself, although, as the members might perceive, it was one of great magnitude.

She was forty-eight years of age, married, and sterile, having never been pregnant, and never had complained of any symptoms referable to the tumour during her lifetime. She never had hemorrhage; of this Dr. Cruise was almost positively certain, as she had been for a long time, indeed, a servant in the house of a family who would have consulted him if she had complained of any affection; and she always appeared to him to enjoy the most excellent health—a circumstance scarcely to be expected in a person in whom so great a change from the normal state existed as in this case.

The tumour is very large; it weighs $4\frac{1}{2}$ pounds, and is, in greatest diameter, $13\frac{1}{2}$ inches, and in its lesser $11\frac{1}{2}$ inches. It does not consist, however, of one single mass, but of an agglomeration of tumours, varying in size from that of an orange to that of a marble. They appear to be developed in a symmetrical way: thus, from the vaginal orifice to the fundus uteri it appears to consist of four masses separated by distinct sulci or depressions; the two larger of them are at the lower part, and two smaller near the fundus; those on the right side are larger than those on the left, as if those latter had not attained their fullest development; and from the other parts several smaller tumours are commencing to grow. I have counted them, and make out thirteen tumours in all; this arrangement gives to the whole mass a lobulated or mamillated appearance, which is rather remarkable.

On making a section of this mass from the os uteri to the fundus, we

observe that the canal, or cavity of the uterus, is completely changed from that of its original shape to that of a mere fissure, and is prolonged to the length of $6\frac{1}{2}$ inches, being encroached upon and lengthened by the development of those masses on each side. It is worthy of remark, however, that the two tumours forming the greatest part of the mass leave a clear line or space between them, showing the original uterine structure, and proving that the metamorphosis must have commenced at different points of the uterine parietes.

On examining a section of the largest of the tumours its structure is well seen; it consists of a number of bands, some of which have a concentric and others an interlacing arrangement; this arrangement of the fibres is very well seen on examination with a lens. The tumour, on section, is very dense, has a firm, gristly feel, and is almost as strong as, and very like, the intervertebral cartilage; yet it does not present any trace of the calcareous degeneration so often seen in those neoplasmata, and of which some examples have been on different occasions exhibited to the society. This calcareous degeneration, according to Scanzoni, Becquerel, and others, may be looked upon as a natural process of cure, or rather as a cessation of development in tumours of this class, and is very often seen in cases where the person the subject of it is old, and they become so gritty and friable that they can be broken by the fingers.

The size to which these fibroids sometimes grow is very great. Thus Gauthier de Claubry mentions one which was 31 inches in its great, and 20 inches in its lesser circumference, and weighed 39 pounds; and Dr. Hewitt exhibited one at the Obstetrical Society of London which weighed 42 pounds, and was 44 inches in circumference. And I saw an instance of one of great magnitude, whilst assistant in the hospital, in which pregnancy occurred. The case is related in Dr. M'Clintock's work upon diseases of females, page 116, and is very remarkable. The woman came from the country, having been delivered of one child; on her admission, forty-eight hours afterwards, she gave birth to two more children. After her death a large fibrous tumour was seen occupying a considerable part of the anterior wall of the uterus. The case is very remarkable, as proving the possibility of the coexistence of, not only conception, but also of plural pregnancy, with fibrous tumour of the uterus of considerable magnitude.

Hemorrhage or metrorrhagia is in general a symptom which shows itself at some period or other during the development of fibroid tumour, and in almost all the cases of this affection which I have seen it was a concomitant symptom; yet it is very remarkable that on no occasion did she complain of this to Dr. Cruise, who, as I have stated before, knew her for a very long time, and in all probability would have been made aware of the circumstance had it occurred.—*9th June, 1866.*

**TRANSACTIONS OF THE COUNTY AND CITY OF CORK
MEDICAL AND SURGICAL SOCIETY.***

SESSION 1864-65.

PRESIDENT in the Chair.

DR. MACNAUGHTON JONES exhibited an abnormal kidney that came under his observation in the dissecting room at the Queen's College. The kidney had shrunk, consequent on the time that had elapsed since its removal, as also from its having been preserved in spirit. The subject (a male) apparently about middle age, presented some abnormalities in his arterial system elsewhere; and on looking at the sketch, as taken by

a Aorta. b Renal vessels. c d Ureters. e Renal vessels. f Cyst.
g Renal artery. A Vein.

Mr. Heazle, it will be seen that the aorta curved suddenly underneath the kidney, from left to right, and consequently the iliacs deviated from their usual course and direction, the left being much the longer and most curved. The kidney is that form alluded to by Dr. Rokitsansky as a very rare one indeed, approaching as nearly as possible to a complete amalgamation of the two organs, lying on the right of the spinal column, extending from the eleventh rib to the third lumbar vertebra, crossing the vena cava and aorta, and lying over, at its extremity, the vertebral column. There is on its anterior surface, and extending back to its

* These reports are supplied by Dr. Curtis, Secretary to the Society.

posterior, a sulcus, apparently the original marking of its division into two distinct organs.

The kidney received two renal arteries, the right one bifurcating and entering in company with its vein at the hilus, where one ureter emerges. The left one, longer than the other, descended in front of the aorta, and entered by itself abruptly to the left, its corresponding vein not accompanying it, but emerging directly from the convex surface of the kidney. There is not the slightest breach of continuity at the sulcus spoken of in the structure of the kidney; one of the ureters appears to emerge directly from the substance of the kidney, not joining the other, but pursuing a separate course over the convex aspect of the kidney, parallel with its fellow to the bladder. In the sketch the posterior surface of the organ is represented, and on the kidney, as delineated in it, two depressions for the vena cava and aorta. Examining the kidney on this surface there will be perceived a triangular depression or hollow, apparently the remains of a small cystic tumour.

I thought that as a form of kidney of which I have not seen much notice taken it might be interesting to draw your attention to it; and should be glad if by any means I might learn some particulars of the man's history while at hospital.—*25th April, 1866.*

DR. JOHNSTON, surgeon, Cork District Military Prison, exhibited pathological specimens of phthisical cavities, hepatized lung, acute pericarditis, and Bright's disease, taken from a soldier who died in the Garrison Hospital, on the 21st February, 1866.

This soldier was thirty-five years of age; eighteen years' service, and of tolerably temperate habits. He had served nine years in the Cape Colony, and four years in India.

While serving in India he suffered from attacks of hepatic disease, and was proposed for discharge from the service in 1861, in consequence of repeated attacks of chronic rheumatism.

He was stationed at Hythe, in 1865, and in April of that year he was attacked with cough and hemoptysis, soon followed by loss of flesh, expectoration, and other symptoms of phthisis, for which he was invalided, and sent to the Cork Garrison Hospital, where he was admitted on the 13th of December last, when his condition was noted to be as follows:—

He was much emaciated and feeble, the pulse rapid and compressible, both legs œdematous; urine scanty, deep coloured, and highly albuminous, specific gravity 1010. Percussion elicited a duller sound than natural in both sub-clavicular regions, and muco-crepitant râles were audible on the application of the stethoscope over the apices of both lungs anteriorly and posteriorly. He coughed frequently, and expectorated copiously, the sputa being flocculent. The skin at times, the patient stated, was moist; but on admission was rough, dry, and harsh as parchment. For weeks

prior to admission the bowels were confined, and frequently required the administration of purgatives. No material change occurred in these symptoms until about ten days prior to his death, when the expectoration became muco-purulent, intense thirst set in, which required a large quantity of fluid daily for its alleviation. The anasarca increased, the abdomen became tympanitic, the pulse rapid and feeble, dyspnea became urgent, and he died, exhausted, on the 21st of February, neither coma nor convulsions having supervened.

The treatment adopted included the occasional administration of hydragogue purgatives, diaphoretics, the hot-air bath, preparations of iron in combination with quinine, and cod-liver oil, &c., with support by means of ale, wine, eggs, broths, &c., &c., given in small quantities and at short intervals.

Autopsy.—The body was much emaciated, and the lower extremities œdematous. Both lungs contained masses of tubercular deposition, both indurated and softened, and were adherent to the parietes of the chest posteriorly, by very strong, broad bands, the result of an old pleuritic attack.

An irregularly shaped cavity, with soft, rough walls, was found in the apex of the left lung, traversed by several fibrous-looking cords, the remains of obliterated bronchial tubes and vessels.

There was a smaller abscess in the apex of the right lung, containing comparatively healthy undecomposed pus. This abscess was circumscribed, and lined with a small white pyogenic membrane, intimately adherent to the surrounding lung tissue, and did not communicate with any bronchial tube. The posterior half of the inferior lobe of right lung was hepatized, and was probably the immediate cause of death. The pericardium contained about 8 oz. of transparent straw-coloured fluid; the heart was soft, somewhat enlarged, and weighed 12 oz.; its surface was rough, and covered with a layer of scrofulous-looking lymph, the result, evidently, of a recent pericardiac attack.

Both mitral and aortic valves were free from disease. The spleen weighed 12 oz., was enlarged, congested, soft, and friable. The liver was normal in size and appearance, and weighed 4 lb. 1 oz.; on the anterior or convex surface of the right lobe there was a well marked cicatrix, with loss of substance, and thickening of the capsule, the result, probably, of an abscess which occurred during his service in India, although no cicatrix corresponding with the site of the supposed abscess could be detected in the abdominal wall. Both kidneys were soft, congested, and flabby, of a whitish colour, and considerably enlarged; the right weighed 9 oz., the left 8 oz.; both presented well marked examples of the second stage of the inflammatory form of Bright's disease.

Remarks.—This case possesses some interest, as it illustrates the cause of death in many cases of Bright's disease, viz., by the supervention of

acute visceral inflammation before the disease of the kidneys advances sufficiently to cause death by uremic poisoning.

The order of sequence of the organic diseases in this case appears to have been phthisis in the first instance, induced, probably, by exposure to vicissitudes of temperature and climate, in a constitution predisposed by the scrofulous diathesis to take on diseased action of a specific character. The nephritic disease then set in, probably not more than about three months antecedent to his death, judging from the pathological condition of the kidneys.

The pericardiac inflammation was not evidenced by any localized symptom ; it appeared to be of a sub-acute character, and to have set in very insidiously—probably three or four weeks prior to the fatal termination of the case ; and owing to effusion taking place at a very early stage of the disease no friction-sound was detected.

The pneumonic complication took place about five days previous to death, and although limited to the base of the right lung was the immediate cause of death in consequence of his previously debilitated condition.—
28th February, 1866.

CLINICAL RECORDS.

Case of Poisonous Effects Produced by Exposure to the Fumes of Tobacco, and Excessive Use of "the Weed." By T. H. BABINGTON Surgeon Londonderry Infirmary.

Patrick Collier, aged thirty, visited by Dr. Harkin, London (whom I am indebted for the accurate notes of the case prior to patient's admission to County Infirmary). On the evening of February 18th, whilst passing his urine, was attacked with sudden violent pain in his head, followed immediately by vomiting; the pain so violent he screamed for assistance; he was found pale, covered with a clammy sweat, pupils contracted, irresponsive pulse 48, small and feeble; he was perfectly conscious; complained of the pain in his head, his voice was feeble and low, he was suffering from intense nervous prostration. He admits having been addicted to drinking ardent spirits to excess, and to an inordinate use of tobacco, and at the same time was exposed for several hours to the fumes arising from tobacco while being "stoved." He never had any other complaint, but he has a slight stricture of the urethra. Moderate medicinal leeches were administered, with heat to the extremities, applied to induce reaction, and a purgative of calomel and jalap ordered.

February 19th.—His head felt easier, pulse 60, skin warm, but the pain is now most intense when he makes water; that it comes with sudden and intense violence just as micturition begins. The urine is dark coloured, s. g. 118, non-albuminous; pupils still contracted, very obedient to light. He was cupped on the nape of neck to extend over four ounces, and ordered three grains iodide potassium and ten grains potassæ, thrice daily, with infusion of gentian; blisters applied to the ears. During the few following days the pain in head, as before, was remitted slightly, but was invariably materially increased on his assuming the upright posture and at micturition. He never complained of any pain or tenderness in the region of the kidneys, bladder, or track of the ureters.

February 22.—His pupils were widely dilated, and obedient to light.

February 26.—He was admitted into County Infirmary, precisely in the condition above described, viz.—pulse 60, small and feeble, countenance anxious and distressed; pupils dilated; little sleep at night; head in paroxysms, and greatly increased at micturition; tongue covered with a white fur. The only change made in the treatment was adopted by Dr. Harkin was substituting the bromide for

potassium ; a blister was applied to the vertex of the head, and he was ordered coffee for breakfast and supper.

27th.—He awoke suddenly in the night with excruciating pain in his head, which continued with violence for an hour.

28th.—Not in so much pain ; his pulse 60 ; he referred the pain to the right side of his head, under the parietal bone.

He improved very slightly, if at all, till March 6th, when at the morning visit, on being examined rather more minutely than usual, in order if possible to detect any symptom which had escaped observation, a strong smell of nicotine was observed ; and on smelling his hands, arms, neck, &c., nicotine was readily recognized pervading the cutaneous surface, evidently excreted with the perspiration. He was directed to continue his medicine, viz.—five grains bromide of potassium three times a day, and to have a vapour bath for half an-hour at bed time. On the evening of the 7th, when in the vapour bath, the patients in the ward all recognized the odour of tobacco. The vapour baths were continued each night till the 12th March, when he left the hospital, at his own desire, much improved.

Ten days after his discharge he was seen by Dr. Harkin ; the smell of nicotine was still perceptible on his skin, and he occasionally suffered from pain in his head. Dr. Harkin had given him several vapour baths.

11th. April.—He was seen by Dr. Harkin, much improved in appearance ; he still suffered occasionally from the pain in his head ; the odour of nicotine was not perceptible.

It is well to remark that our senses of smell were in no way deceived, for we carefully distinguished between the smell of tobacco oil on the clothes and the odour of nicotine on the skin.

INDEX

TO THE FORTY-SECOND VOLUME.

- Air, epidemic constitution of, 135
 Amputation, at hip joint, 297—tying of veins in, 465.
 Anatomy, atlas of surgical and topographical, by Beraud, *Rev.*, 167
 Aneurism of aorta, 402, 474, 512.
 Annandale, malformations, diseases, and injuries of the fingers and toes, and their surgical treatment, *Rev.*, 426.
 Anteversion of uterus, 116.
 Amyloid substances, formation and destination of, 162—degeneration, 475, 502.
 Aorta, aneurism of, 402, 474, 512.
 Aphasia, 439, 445, 488, 507.
 Army hygiene, by Gordon, *Rev.*, 416.
 Asthma, chloroform in, 84.
- Babington, on flax mills, their machinery, accidents occurring therein, with suggestions for their prevention, 392—case of poisoning by fumes of tobacco, 544.
 Bacon, on cancer of the brain, *Rev.*, 466.
 Banks, case of hemiplegia with loss of speech, 488.
 Barton, on results of stricture, 501.
 Beale, new observations on structure of nerve centres, *Rev.*, 128.
 Beatty, contributions to medicine and midwifery, *Rev.*, 397
 Beck, on microscope, *Rev.*, 414.
 Belcher, Memoir of Sir P. Dun, by, 231—*Rev.*, 413
 Bennett, patent foramen ovale, 174—tumour of uterus, 493.
 Bevan report on scalds of the larynx, 367.
 Billing, on the treatment of cholera, *Rev.*, 403.
 Bladder, calculus in, 480—rupture of, 485, 499.
 Bone, necrosis of inferior maxilla, 172—exostosis of vomer, 334—necrosis of superior maxilla, 337.
 Bonnafont, le cholera et la congrès sanitaire, *Rev.*, 132.
- Brain, inflammation of, distinguished from fever, by temperature, 65—new observations on structure of nerve centres, *Rev.*, 128—symptoms and diagnosis of tumours of, *Rev.*, 435—cancer of, 466—ossification of dura mater, 486—softening of, 507.
 Butcher, on amputation at hip joint, 297.
 Byrne, case of puerperal fetid pulmonary abscess, 210—case of rupture of vagina during labour, 537—large fibrous tumour of uterus, 539.
- Cancer, visceral, 169—of uterus, mode of death in, 401—works on, *Rev.*, 467.
 Cancrum oris, operation to repair deformity from, 328.
 Cerebro spinal meningitis, temperature in, 62, 65.
 Chloroform, medical uses of inhalation, 83—for gall stone, 83—in asthma, 84—in whooping cough, 84—epilepsy, 85—as an aid to the taxis 95—in midwifery, 399—in operations for cleft palate, 425.
 Cholera, origin of, 133—conditions necessary for spread of, 135, 141—portability of, 138, 404—origin of recent epidemic, 138, 404—origin of, 133—elimination in, 146, 410—pathology of, 145, 152—treatment, 145, 156, 405—recent works on, *Rev.*, 132, 403—venesection in, 408—use of sulphites in, 411.
 Chromic acid, use of for granular erosion of, in uterus, 111.
 Cockle, on intra-thoracic cancer, *Rev.*, 466.
 Codex-medicamentarius — pharmacopée Française, *Rev.*, 447.
 Collis, M. H., contributions to operative surgery, Part I., operations about the face, 324.
 Compton, T. A., on temperature in acute diseases, 60.
 Conception, obstacles to, 108—conditions essential to, 108.

- Cooke, on cancer, its allies and counterfeits, *Rev.*, 466.
 Cork med. and surg. society, transactions of, 194, 541.
 Corvisart, memoirs on a function of the pancreas little known, *Rev.* 120.
 Croly, necrosis of lower jaw, 172.
- Delirium tremens, digitalis in, 200.
 Denham, case of acute inversion of uterus after delivery, 181—fibrous tumour of uterus, 498.
 De Ricci, on the use of sulphite of magnesia in the treatment of zymotic disease, 360.
 Digitalis, uses in med. practice, 194—active principles of, 196.
 Diphtheria, Hayden, on use of hyposulphites in, 86.
 Disease, temperature in acute, 60—use of sulphites in zymotic, 360.
 Dobell, on nature, cause, and treatment of tuberculosis, *Rev.*, 120.
 Drysdale, on cholera, its nature and treatment, *Rev.*, 403.
 Dun, memoirs of Sir P., 231—*Rev.*, 413.
 Duodenum, laceration of, 497.
 Dura mater, ossification of, 478.
 Duncan, case of hepatic abscess, 491.
 Dysmenorrhea, 115, 402.
- Ecraseur, Sims' porte chain for, 113.
 Epidemics, Smith on common nature of, *Rev.* 132, 136.
 Epilepsy, chloroform in, 85—digitalis in, 201.
 Epithelioma, of nose, 326—of mouth and cheek, removal, 329, 330—of tongue, 338.
 Ergot, of rye, influence of, on fetus, 399—use of in midwifery, 399.
 Erysipelas, temperature in, 64.
 Eustace, case of ossification of dura mater, 478.
 Eye, ophthalmia of new born infants, 184—mode of examining in infants, 188—examination of with ophthalmoscope, 345—staphyloma, 495.
- Face, Collis on operations about, 324.
 Fetus, influence of ergot of rye on, 399.
 Fever, diagnosis of typhus from cerebral affections by thermometer, 65—scarlet, temperature in, 64—course of temperature and pulse in typhus and typhoid, 71.
 Fistula, vesico vaginal, 402.
- Fingers, malformations, diseases, and injuries of, and toes, Annandale on, *Rev.*, 426—operation for webbed, 427—injury of digital nerves, 429—excision of metacarpo phalangeal joints, 434.
 Flax-mills, their machinery, accidents occurring therein, with suggestions for their prevention, 392.
 Fleming, case of lithectasy, 480—polypus of rectum, 487—fracture of pelvis, 499.
 Forceps, use of, in difficult labour, 398.
 Foster on the use of the sphygmograph in the investigation of diseases, *Rev.* 125.
 Fracture, of vertebrae, 1—of skull, 173—of pelvis, 485, 499.
- Gangrene, senile, 433.
 Glycerine, use of, in affections of uterus, 112.
 Gordon, C. A., on army hygiene, *Rev.* 416.
 Gordon, S., case of trephining of the spine, 2.
 Greenhow, on causes of cholera, 141.
 Griffith, on microscope, *Rev.*, 414.
- Hamilton, E., case of intra-thoracic aneurism, 474.
 Hamilton, on injuries of nerves, 431.
 Hayden, on diphtheria, 86—case of cardiac disease, 179—on visceral cancer, 169—softening of brain, 507—aphasia, 507—aneurism of aorta, 512—cirrhosis of liver, 519.
 Heart, on the action of, 35—ganglia of 130—mitral obstruction, aortic regurgitation, 197—patent foramen ovale 174—disease of valves of, 504, 505, 517, obesity of, 511.
 Heat, of body, normal, 60—in disease, 61.
 Hernia, strangulated inguinal, 94—use of chloroform, 95—congenital, 98.
 Hip, amputation at, 299.
 Hooping cough, chloroform in, 84.
 Hygiene, army, by Gordon, *Rev.*, 416.
- Influenza, preceding epidemic of cholera, 136.
 Insanity, and criminal responsibility, 221, 523—moral, 229—ossification of dura mater in a case of, 486.
 Iron, styptic salts of, 112.
 Isdell, case of rupture of uterus, recovery 537—on placenta previa, 537.
- Jennings, case of disease of heart, 550, 517.

- Johnson, G., on cholera, its nature and treatment, *Rev.* 132, 145—on epidemic diarrhea and cholera, *Rev.*, 403.
- Johnston, case of diseased lung, heart, and kidneys, 542.
- Joint, amputation at hip, 297.
- Jones, on digitalis and its uses, 194—abnormal kidney, 541.
- Joynt, case of protracted utero-gestation, with remarks, 377.
- Kidd, Chas., on medical uses of chloroform inhalation, 83.
- Kidd, G. H., on an improved form of medicated pessary, 537.
- Kidney, abnormal form, 541—Bright's disease of, 482.
- Kingsford, on a new theory of cholera, *Rev.*, 403.
- Labour, use of instruments in difficult, 398—ergot of rye in, 399—complicated with a tumour in the pelvis, 400—rupture of vagina during, 539—complicated with peritonitis and ascites, 537.
- Ladame, symptoms and diagnosis of tumours of the brain, *Rev.*, 435.
- Laryngitis, chronic, 171.
- Larynx, report on scalds of, 367.
- Law, on obesity of heart, 511.
- Lectures, clinical, by Solly, *Rev.*, 463.
- Lithectasy, case of, 480.
- Liver, use of chloroform in gall stones, 83—M'Donnell on functions of, *Rev.*, 162—abscess of, 491—amyloid degeneration of, 501—rupture of biliary duct, 518—cirrhosis of 519.
- Lunatic, criminal autobiography of a, 224.
- Lung, puerperal fetid abscess, 210—pneumothorax without perforation, 202—pneumonia, 64.
- Lyons, case of Bright's disease, 482.
- Madden, on insanity and criminal responsibility, 224, 523.
- Mania, puerperal, symptomatic of blood poisoning, 215—and criminal responsibility, 224, 523.
- Materia medica, S. Jackson's note book of, *Rev.*, 411—French pharmacopeia, *Rev.*, 447.
- M'Clintock, case of double monstrosity, 192.
- M'Cormac, reports of hospital cases; strangulated inguinal hernia, 94.
- M'Donnell, R., on the operation of trephining in cases of fracture of the spine, 1—on functions of liver and formation and destination of amyloid substances, &c., *Rev.*, 162—on chronic laryngitis, 171—fracture of skull, 173.
- Medicine and midwifery, contributions to by Beatty, *Rev.*, 397.
- Menorrhagia, 110—use of digitalis in, 199.
- Menstruation, definition of healthy, 110—causes of profuse, 110—painful, 115, 402.
- Microscope, recent works on, *Rev.* 414.
- Midwifery and medicine, contributions to, by Beatty, *Rev.*, 397—use of instruments in, 398—ergot of eye in, 399—chloroform in, 399.
- Monster, double, 192—delivery of, 193—diagnosis of, 194.
- Moore, on antecedents of cancer, *Rev.*, 467.
- M'Pherson, cholera in its home, *Rev.*, 132.
- M'Swiney, on rupture of biliary duct, 518.
- M'Swiney, case of recovery from traumatic tetanus, 219.
- Murder and suicide, attempted by a lunatic, 224.
- Muscle, Ranke, researches on condition of in tetanus, 450.
- Nails, ingrowing, 428—horny, 428.
- Necrosis of lower jaw, 172—of upper jaw, 337.
- Nerve, new observations on structure of centres, *Rev.*, 128—division of gustatory, injury of digital, 429.
- Nose, lupoid destruction of, rhinoplasty, 324—epithelioma of, rhinoplasty, 326—fibro-plastic growth from, 331—exostosis of vomer, 334.
- Obstetrical Society of Dublin, proceedings of, 181, 537.
- Ophthalmia of new born infants, 184.
- Ophthalmoscope, on the theory and principles of, 345.
- Osteo-sarcoma of leg and thigh, 297.
- Palate, Warren on fissure of hard and soft, *Rev.* 424.
- Parkin on antidotal treatment of cholera, *Rev.*, 483.
- Pancreas, Corvisart on digestion of azotized aliment by, 120—defect in action the cause of tuberculosis, 123.
- Priapism in injuries of spine, 28.
- Paton, G., on the action of the heart, 35.
- Pathological Society of Dublin, proceedings of, 168, 474.

- Pellaria, le choléra ou typhus Indien
 epidemie de, *Rev.*, 132.
 Peritonitis, frottement in, 402.
 Pessaries, medicated, improved form of,
 537.
 Pharmacopeia, —new French, *Rev.* 447.
 Physicians, transactions of the Medical
 Society of, 210, 523—O charter of Col-
 lege of, 249—liability to cholera, 145.
 Placenta previa, 537.
 Pneumonia, temperature in, 64—tym-
 panitic resonance in, 206.
 Pneumothorax without perforation, 202.
 Polypus of uterus, 112—of rectum, 487.
 Pregnancy, case of protracted, 377—
 complicated, with a tumour in the
 pelvis, 401—with peritonitis and as-
 cites, 537.
 Procidentia of uterus, 117.
 Prolapse of uterus, 117, 402.
 Pubes, fracture of, 485.
 Purser, case of amyloid degeneration,
 475, 502—on staphyloma, 495.

 Quarantine in cholera, 140.
 Quickening, date of, in pregnancy, 380.

 Ranke on tetanus, *Rev.*, 450.
 Record, clinical, 544.
 Rectum, polypus of, 487.
 Retroversion of uterus, 116.
 Rheumatism, temperature in acute, 64.
 Rhinoplasty, Collis on, 324.
 Ringland, case of pregnancy complicated
 with peritonitis and ascites, 537.

 Sansom, on arrest and prevention of
 cholera, *Rev.*, 403.
 Scalds of larynx, 367.
 Scarlatina, temperature in, 64.
 Scoresby-Jackson, note-book of materia
 medica, *Rev.*, 411.
 Sedgwick, on nature of cholera as a guide
 to treatment, *Rev.*, 403.
 Septicemia, temperature in, 62.
 Sims, clinical notes in uterine surgery,
 by, *Rev.*, 107.
 Skull, fracture of 173.
 Smith, on epidemics, *Rev.*, 132.
 Snow, on cause of cholera, 142.
 Solly, surgical experience, *Rev.*, 463.
 Sound, uterine, 109-117.
 Speech, loss of in disease of brain, 439,
 445, 488, 507.
 Sphygmograph, Foster on use of in the
 investigation of disease, *Rev.*, 125.

 Spine, trephining in fracture, 1—diagnosis
 between laceration and compression of
 cord, 25—reflex movements in injury
 of, 27—softening of cord in three
 forms, 30.
 Spitta, brief remarks on cholera, *Rev.*,
 403.
 Sponge tents, 112.
 Staphyloma, 495.
 Sterility, Sims on, *Rev.*, 107.
 Story on cholera, *Rev.*, 132.
 Stricture, use of catheters in, 465—results
 of long-continued, 501.
 Suicide and murder attempted by a
 lunatic, 224.
 Sulphites, use of in zymotic disease,
 360—in cholera, 411.
 Surgery, clinical notes in uterine, by
 Sims, *Rev.*, 107—contributions to op-
 erative, by M. H. Collis, 324—clinical
 lectures, by Solly, *Rev.*, 463.
 Symes, case of separation of symphysis
 pubis and rupture of bladder, 485—
 laceration of duodenum, 497.

 Temperature, on the practical value of
 daily observations on in acute diseases,
 60—normal of body, 61—increase of in
 injury of spine, 28—in typhus and
 typhoid, 64, 71—in pneumonia, 64—in
 erysipelas, 64—in acute rheumatism,
 64—in tonsillitis, 64—in scarlet fever
 64.
 Tetanus, case of recovery from traumatic
 219—a physiological study, by Rankin
Rev., 450.
 Thermometer, use of in reference to acute
 disease, 60.
 Tobacco, poisonous effects of, 544.
 Toes, malformations, injuries, and diseases
 of and fingers, Annandale on, *Rev.*, 42.
 Tongue, operations for epithelial diseases,
 338—division of gustatory nerve, 341.
 Tonsillitis, temperature in, 64.
 Townsend, E. R., case of pneumothorax,
 without perforation, 202.
 Tuberculosis, Dobell on, *Rev.*, 120.
 Tucker, on cholera, *Rev.*, 132.
 Typhus and typhoid, range of temperature
 in, 71.

 Ulcer, fibro-plastic, of mouth, 331.
 Urethra, stricture of, 465, 501.
 Utero-gestation, case of protracted, 377.
 Uterus, clinical notes on surgery of, by
 Sims, *Rev.*, 107—examinations, 109—
 treatment of granular erosions, 111—
 polypi of, 112, use of glycerine in
 affections of, 112—obstruction of os,
 115, 402—malpositions of, 116, 401—

Index.

- use of the sound, 109, 117—proci-
dencia, 117, 402—longitudinal hyper-
trophy, 118—acute inversion after deli-
very, 181—action of digitalis on, 199—
mode of death in cancer of, 401—rup-
ture, recovery, 537—large fibrous
tumour of, 493, 498, 537.
- Vagina, rupture of during labour, re-
covery, 537.
- Vaginismus, 119.
- Veins, tying of in amputations, 465.
- Vertebrae, fracture of, 1.
- Vomer, exostosis of, 1
operation, 334.
- Warren, on fissures
palate, *Rev.* 424.
- Water, impura, a cause
Whitlow, 428.
- Wilson, on ophthal-
infants, 184—on the
principles of the ophthal-
Wise, cholera, its syn-
remedies, *Rev.*, 187.

41.C
348

